PROCEEDINGS

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NINETY-SEVENTH SESSION.

Monday, 5*th April* 1880.

SIR WYVILLE THOMSON, Vice-President, in the Chair.

The following Communication was read:—

1. On the Structure and Origin of Coral Reefs and Islands. By John Murray.

(*Abstract.*)

Darwin's Theory.—During the voyage of the "Beagle" and subsequently, Mr Darwin made a profound study of coral reefs, and has given a theory of their mode of formation which has since been universally accepted by scientific men.

Darwin's theory may be said to rest on two facts—the one physiological, and the other physical—the former, that those species of corals whose skeletons chiefly make up reefs cannot live in depths greater than from 20 to 30 fathoms; the latter, that the surface of the earth is continually undergoing slow elevation or subsidence.

The corals commence by growing up from the shallow waters surrounding an island, and form a fringing reef which is closely attached to the shore. The island slowly sinks, but the corals continually grow upwards, and keep the upper surface of the reef at a level with the waves of the ocean. When this has gone on for some time a wide navigable water channel is formed between the

reef and the shores of the island, and we have a barrier reef. These processes have but to be continued some stages further, when the island will disappear beneath the ocean, and be replaced by an atoll with its lagoon where the island once stood.

According to this simple and beautiful theory, the fringing reef becomes a barrier reef, and the barrier reef an atoll by a continuous process of development.

Object of the Present Paper.—Professor Semper,* during his examination of the coral reefs in the Pelew group, experienced great difficulties in applying Darwin's theory. Similar difficulties presented themselves to the author in those coral reef regions visited during the cruise of the "Challenger."

The object of the present paper is to show, *first*, that, while it must be granted as generally true that reef-forming species of coral do not live at a depth greater than 30 or 40 fathoms, yet that there are other agencies at work in the tropical oceanic regions by which submarine elevations can be built up from very great depths so as to form a foundation for coral reefs; *second*, that while it must be granted that the surface of the earth has undergone many oscillations in recent geological times, yet that all the chief features of coral reefs and islands can be accounted for without calling in the aid of great and general subsidences.

Nature of Oceanic Islands and Submarine Elevations.—It is now known that, with scarcely an exception,† all oceanic islands other than coral atolls are of volcanic origin. Darwin, Dana, and others have noticed the close resemblance between atolls and ordinary islands in their manner of grouping as well as in their shapes. In a previous paper the author pointed out the wide distribution of volcanic debris over the bed of the ocean in tropical regions, and the almost total absence of minerals, such as quartz, which are characteristic of continental land.‡ There is every reason for believing that atolls are primarily situated on volcanic mountains and not on submerged continental land as is so often supposed.

* Zeitschr. für Wissen. Zoologie, vol. xiii. p. 563.

† New Zealand, New Caledonia, and the Seychelles have primitive rocks, if these can be regarded as oceanic islands. Some of the islands between New Caledonia and Australia may have primitive rocks, and the atolls in these regions may be situated on foundations of this nature.

‡ Proc. Roy. Soc. Edin., 1876–77, p. 247.

The soundings of the "Tuscorora" and "Challenger" have made known numerous submarine elevations: mountains rising from the general level of the ocean's bed, at a depth of 2500 or 3000 fathoms, up to within a few hundred fathoms of the surface. Although now capped and flanked by deposits of Globigerina and Pteropod ooze, these mountains were most probably originally formed by volcanic eruptions. The deposits in deep water on either side of them were almost wholly made up of volcanic materials.

Volcanic mountains situated in the ocean basins, and which during their formation had risen above the surface of the water, would assume a more or less sharp and pointed outline owing to the denuding action of the atmosphere and of the waves, and very extensive banks of the denuded materials would be formed around them. Some, like Graham's Island, might be wholly swept away, and only a bank with a few fathoms of water over it be left on the spot. In this way numerous foundations may have been prepared for barrier reefs and even atolls.

Those volcanoes which during their formation had not risen above the surface of the sea (and they were probably the most numerous) would assume a rounded and dome-like contour,* owing to the denser medium into which the eruptions had taken place, and the deposits which had been subsequently formed on their summits.

In order to clearly understand how a submarine mountain, say half a mile beneath the sea, can be built up sufficiently near the surface to form a foundation on which reef-forming corals might live, it is necessary to consider attentively the

Pelagic Fauna and Flora of Tropical Regions.—During the cruise of the "Challenger," much attention was paid to this subject. Every day while at sea tow-nets were dragged through the surface waters; and while dredging they were sent down to various depths beneath the surface. Everywhere life was most abundant in the surface and sub-surface waters. Almost every haul gave many calcareous, siliceous, and other Algæ; great numbers of Foraminifera and Radiolaria, Infusoria, Oceanic Hydrozoa, Medusæ, Annelids; vast numbers of microscopic and other Crustacea, Tunicates, Pelagic Gastropods, Pteropods, Heteropods, Cephalopods,

* Scrope on Volcanoes, chap. viii.

Fishes, and fish-eggs; larvæ of Echinoderms, and of many of the above creatures, &c.

Most of these organisms live from the surface down to about 100 fathoms.* In calm weather they swarm near the surface, but when it is rough they are to be found several fathoms beneath the waves. They are borne along in the great oceanic currents which are created by the winds; and meeting with coral reefs, they supply the corals on the outer edge of the reefs with abundant food. The reason why the windward side of a reef grows more vigorously appears to be this abundant supply of food, and not the more abundant supply of oxygen as is generally stated. The "Challenger" researches showed that oxygen was particularly abundant in all depths inhabited by reef-forming corals.

When these surface animals die, either by coming in contact with colder water or from other causes, their shells and skeletons fall to the bottom, and carry down with them some organic matter which gives a supply of food to deep-sea animals. The majority of deep-sea animals live by eating the mud at the bottom.

An attempt was made to estimate the quantity of carbonate of lime, in the form of calcareous Algæ, Foraminifera, Pteropods, Heteropods, Pelagic Gastropods, in the surface waters. A tow-net, having a mouth 12½ inches in diameter, was dragged for as nearly as possible half a mile through the water. The shells collected were boiled in caustic potash, washed, and then weighed. The mean of four experiments gave 2·545 grammes. If these animals were as abundant in all the depth down to 100 fathoms as they were in the track followed by the tow-net, this would give over 16 tons of carbonate of lime in this form in a mass of the ocean one mile square by 100 fathoms.†

* The Challengeridæ, and many of the other members of Haeckel's new order *Phæodaria*, certainly live deeper, as we never got them in the tropics except when the net was sent down to a depth of 200 or 300 fathoms.

† Among the varieties of Foraminifera recognised by Mr Brady in the "Challenger" collections, the following have a Pelagic habitat:—

Pulvinulina Menardii.	*Pullenia obliquiloculata.*
,, *canariensis.*	*Sphærodina dehiscens.*
,, *crassa.*	*Caudeina nitida.*
,, *Micheliniana.*	*Hastigerina Murrayi.*
,, *tumida.*	,, *pelagica.*

Bathymetrical Distribution of the Calcareous Shells and Skeletons of Surface Organisms.—Although these lime-secreting organisms are so abundant in tropical surface waters, their cast-off shells and skeletons are either wholly or partially absent from by far the greater part of the floor of the ocean. In depths greater than 3000 fathoms we usually met with only a few shells of Pelagic Foraminifera of the larger and heavier kinds; a few hundred fathoms nearer the surface they became more numerous, and we get a few of the smaller kinds and some Coccoliths and Rhabdoliths. At about 1900 or 1800 fathoms a few shells of Pteropods and Heteropods are met with; and in all depths less than a mile we have a deposit in which the shell and skeletons of almost every surface organism is to be found. In the equatorial streams and calms the calcareous Algæ, Pelagic Foraminifera, Pteropods, and Heteropods are more abundant on the surface than elsewhere; and it is in these same regions that we found their dead shells at greater depths than in

Orbulina universa.	*Globigerina dubia.*
Globigerina bulloides.	,, *rubra.*
,, *æquilateralis.*	,, *conglobata*
,, *sacculifera* (hirsuta).	,, *inflata.*

It is the dead shells of these Pelagic Foraminifera which chiefly make up the calcareous oozes of the deep sea. The living shells of all the above varieties swarm in the tropical and sub-tropical waters near the surface. It is especially in the region of the equatorial calms that the largest and thickest shelled specimens are found. As we go north or south into colder water they become smaller, and many varieties die out. In the surface waters of the Arctic and Antarctic regions, only some dwarfed specimens of *Globigerina bulloides* are met with. The author is unable to agree with Dr Carpenter and Mr Brady in thinking that these Pelagic Foraminifera also live on the bottom. This question was made the subject of careful investigation during the cruise. The shells from the surface and from the bottom were compared at each locality, and it was found, by micrometric measurement, that surface specimens were as large and as thick shelled as any average specimens from the soundings. It is quite unlikely that the same individuals should pass a part of their lives in the warm sunny surface waters, at a temperature of from 70° to 80° Fahr., and another part in the cold dark waters two or three miles beneath, at a temperature of 30° or 40° Fahr. The geographical distribution of these Pelagic forms over the bottom coincides exactly with the distribution of the same forms on the surface; that is to say, both on the surface and on the bottom, the distribution is ruled by surface temperature. No specimens of these Pelagic varieties were ever obtained from the bottom with the shells filled and surrounded with sarcode. Whereas creeping and attached forms (like Truncatulina, Discorbina, Anomalina, and some Textulariæ) were taken in this condition in almost every dredge. These last-mentioned forms which we know live on the bottom have a distribution quite independent of surface temperature.

the deposits of other parts of the ocean. Another circumstance influences the bathymetrical distribution of these surface shells. When there is a complete and free oceanic circulation from the top to the bottom, these dead shells are found at greater depths in the deposits than where the circulation is cut off by submarine barriers.

The agent by which these shells are removed is, as Sir Wyville Thomson suggested, carbonic acid. Analysis shows that carbonic acid is most abundant in sea water, and especially so in deep water. Pteropod and Heteropod shells are very much larger than the Foraminifera, yet are very much thinner; and hence, for the quantity of lime contained in them, they present a much greater surface to the action of the sea water. This seems to be the reason why all large and thin shells are first removed from the deposits with increasing depth, and not the fact that some shells are composed of arragonite and some of calcite, as has been suggested.

There is a continual struggle in the ocean with respect to the carbonate of lime. Life is continually secreting it and moulding it into many varied and beautiful forms. The carbonic acid of ocean waters attacks these when life has lost its hold, reduces the lime to the form of a bicarbonate, and carries it away in solution. In all the greater depths of the ocean these surface shells are reduced to a bicarbonate either during their fall through the water or shortly after reaching the bottom.

In the shallower depths—on the tops of submarine elevations or volcanoes—the accumulation of the dead silicious and calcareous shells is too rapid for the action of the sea water to have much effect. Long before such a deposit reaches sufficiently near the surface to serve as a foundation for reef-forming corals, it is a bank on which flourish numerous species of Foraminifera, Sponges, Hydroids, deep-sea Corals, Annelids, Alcyonarians, Molluscs, Polyzoa, Echinoderms, &c. All these tend to fix and consolidate such a bank, and add their shells, spicules, and skeletons to the relatively rapid accumulating deposits. Eventually coral-forming species attach themselves to such banks, and then commences the formation of

Coral Atolls—Mr Darwin has pointed out that "reefs not to be distinguished from an atoll might be formed" * on submerged banks such as those here described. However, the improbability of

* Coral Reefs, p. 118.

so many submerged banks existing in the open ocean caused him to reject this mode of formation for atolls. As here stated, recent deep-sea investigations have shown that submerged banks are continually in process of formation in the tropical regions of the ocean, and it is in a high degree probable that the majority of atolls are seated on banks formed in this manner.

Mr Darwin has also pointed out that the corals on the outer margin of a submerged bank would grow vigorously, whilst the growth of those on the central expanse would be checked by the sediment formed there, and by the small amount of food brought to them.* Very early in the history of such an atoll, and while yet several fathoms submerged, the corals situated on the central parts would be placed at a disadvantage, and this would become greater and greater as the coral plantations approached the surface. When the coral plantation was small there was a relatively large periphery for the supply of food to the inner parts, and also for the supply of sediment; and hence, in small atolls the lagoon was very shallow, and was soon filled up. For the same reasons coral islands situated on long and narrow banks have no lagoons. An atoll one mile square has a periphery of four miles. In an atoll four miles square —the periphery increasing in arithmetical progression and the area as the square—we have for each square mile only a periphery of one mile over which food may pass to the interior, and from which sediment is supplied for filling up the lagoon.

With increasing size, then, the conditions become more and more favourable to the formation of lagoons, and as a consequence we have no large or moderate sized coral islands without lagoons. Townet experiments always showed very much less Pelagic life (food) in the lagoon waters than on the outer edge of the reef. The lagoon becomes less favourable for the growth of all the more massive kinds of coral as the outer edge of the reef reaches the surface, and cuts off the free supply of ocean waters. Many species of corals die.† Much dead coral, coral rock, and sediment is exposed to the solvent action of the sea water. Larger quantities of lime are carried away in solution as a bicarbonate from the lagoon than are

* Coral Reefs, p. 134.

† There are no living corals or shells in some small lagoons, the waters of which become highly heated, and in some cases extremely saline.

secreted by the animals which can still live in it; the lagoon thus becomes widened and deepened.*

On the other hand a vigorous growth and secretion of lime takes place on the outer margins of the reef; and when the water outside becomes too deep for reef-forming corals to live, these still build seawards on a talus made up of their own debris:—the whole atoll expands somewhat after the manner of a Fairy Ring.

It is not necessary to call in disseverment of large atolls in order to explain the appearances presented in the Great Maldiva group of atolls.† The coral fields rising from very many parts of these extensive submarine banks form atolls. The marginal atolls have from the first the advantage of a better supply of food. They elongate in the direction of the margin of the bank where the water is shallower than to seaward. Many of these marginal atolls have coalesced, and as this growth and coalescence have continued, a large part of the food-supply has been cut off from the small atolls situated towards the interior of the bank. Ultimately a large atoll like Suadiva atoll would be formed. The atolls in the interior would be perhaps wholly removed in solution, and the atoll-like character of small marginal but now coalesced atolls would be wholly or partially lost by the destruction of their inner sides.‡ A study of the charts shows all the stages in this mode of development.

In the case of the Lakadivh, Caroline, and Chagos archipelagos we have submarine banks at various stages of growth towards the surface, some too deep for reef-forming species of coral, others with coral plantations, but all submerged several fathoms, and scattered amongst these some of the oldest and most completely-formed atolls and coral islands. It is most difficult to conceive how these sub-

* Complete little Serpula-atolls, with lagoons from 3 to 50 feet in diameter, and formed in this way without subsidence, were numerous along the shores of Bermuda.

† Mr Darwin's application of his theory to this group—where the disseverment of large atolls is called in, and a destructive power attributed to oceanic currents, which it is very unlikely they can ever possess—has often been considered unsatisfactory.

‡ "In speaking of Bow Island, Belcher mentions the fact that several of its points had undergone material change, or were no longer the same when visited after the lapse of fourteen years. These remarks refer particularly to islets situated within the lagoon. I could myself quote many instances of the same description."—"Wilkes' Exploring Expedition," vol. iv. p. 271.

merged banks could have been produced by subsidence, situated as they are in relation to each other and with respect to the perfectly-formed atolls of the groups.

It is a much more natural view to regard these atolls and submerged banks as originally volcanoes reaching to various heights beneath the sea, and which have subsequently been built up to and towards the surface by accumulations of organic sediment and the growth of coral on their summits. It is a remarkable fact that, in all coral atolls which have been raised several hundred feet above the sea, the base is generally described as composed of solid limestone, or "of various kinds of coral evidently deposited after life had become extinct." * This base is probably often made up of such a rock as that brought by the missionaries from New Ireland, and described by Professor Liversidge, † as composed chiefly of Pelagic Foraminifera, the same as those taken by the "Challenger" in the surface waters of the Pacific.

Microscopic sections of a rock taken from 50 feet below sea level at Bermuda show that a deposition of carbonate of lime is going on. The small shells are filled with, and the broken pieces of shells and corals are cemented by, calcite. The wells in coral islands rise and fall with the tide, so that the whole atoll is filled like a sponge with sea water. This water is very slowly interchanged, and by the solution of the smaller and thinner particles, becomes saturated, and a deposition of lime follows. In this way we may explain the absence of many of the more delicate shells from some limestones. ‡

Barrier Reefs.—During the visit of the "Challenger" to Tahiti, a careful examination was made of the reefs by dredging, sounding, &c., in a steam pinnace, both inside and outside the reefs. Lieutenant Swire of the "Challenger" made a careful trigonometrical survey of the profile of the outer reefs on six different lines; and while associated with him in this work, the author was indebted to that officer for many valuable suggestions.

A ledge ran out from the edge of the reef to about 250 yards, where we got a depth of from 30 to 40 fathoms. It was covered with a most luxuriant growth of coral bosses and knobs.

* U. S. Ex. Exp., vol. iv. p. 269. † Geol. Mag., Dec. 1877.

‡ Fuchs, Über die Entstehung der Aptychenkalke. Sitzb. der k. Akad. der Wissensch. 1877.

Between 250 and 350 yards from the edge of the reef there was generally a very steep and irregular slope; about 100 fathoms was got at the latter distance, and the angles between these last-mentioned distances often exceeded 45 degrees. The talus here appeared to be composed of huge masses and heads of coral, which had been torn by the waves from the upper ledge and piled up on each other. They were now covered with living Sponges, Alcyonarians, Hydroids, Polyzoa, Foraminifera, &c.*

From 350 to 500 yards from the edge of the reef, we had a slope with an angle of about 30°, and made up chiefly of coral sand. Beyond 500 yards the angle of the slope decreased till we had at a distance of a mile from the reef an angle of 6°, a depth of 590 fathoms, and a mud composed of volcanic and coral sand, Pteropods, Pelagic and other Foraminifera, Coccoliths, &c.

In the lagoon channel the reefs were found to be fringed with living coral, and to slope downwards and outwards for a few feet, and then plunge at once to a depth of 10 or 16 fathoms. Many portions of these inner reefs were overhanging, and at some places overhanging masses had recently fallen away. Everywhere much dead coral rock was exposed to the solvent action of the sea water. The reefs of Tahiti are at some places fringing, at other places there is a boat passage within the reef, and at Papiete there is a large ship channel with islets within, and the outer edge of the reef is a mile distant from the shore. The island itself is surrounded with a belt of fertile low land, frequently three or four miles wide; this shows that the island has not in recent times undergone subsidence; there are, indeed, reasons for supposing it has recently been slightly elevated. Everything appears to show

* This ledge and steep slope beyond where a depth of 30 or 40 fathoms was reached, was characteristic of a large number of atoll and barrier reefs, and seemed due to wave action. Experiments had been made with masses of broken coral, and it was found that these could (on account of their rough and jagged surface) be built up into a nearly perpendicular wall by letting them fall on each other. A talus formed in water deeper than 40 fathoms where there was little if any motion would be different from one formed on land. In the latter case the disintegrating forces at work always tended to set the talus in motion; in the former case everything tended to consolidate and to fix the blocks in the positions first assumed. A removal of lime in solution would take place from the blocks forming this steep slope, but, except in very deep water, this would not be sufficient to check the outward extension of the reef.

that the reefs have commenced close to the shore and have extended seawards, first on a foundation composed of the volcanic detritus of the island, and afterwards on a talus composed of coral debris, and the shells and skeletons of surface organisms.*

The lagoon channel was subsequently slowly formed by the solvent action of the sea water thrown over the reefs at each tide, and the islets in the lagoon channel are portions of the original reef still left standing. The reefs have extended outwards from the island and have been disintegrated and removed behind in the same way as the atoll has extended outwards after reaching the surface.

Where reefs rise quite to the surface, and are nearly continuous, we find relatively few coral patches and heads in the lagoons and lagoon channels. Where the outer reefs are much broken up, the coral growths in the lagoon are relatively abundant. Where the water was deep and the talus to be formed was great, the outward growth has been relatively slow,† and the disintegrating forces in the lagoons and lagoon channels gaining in the struggle, the reefs would become very narrow and might indeed be broken up. This, however, would admit the oceanic waters and more food, and growth would again commence on the inner as well as the outer sides of the still remaining portions. In the great barrier reef of Australia, where the openings are numerous and wide, the reefs have a great width. Where the openings are few and neither wide nor deep (as in lat. 12° 30′) the reefs are very narrow and "steep to"—on their inner side.

At the Admiralty Islands, on the lagoon side of the islets on the barrier reefs, the trees were found overhanging the water, and in some cases the soil washed away from their roots. It is a common observation in atolls that the islets on the reefs are situated close to the lagoon shore. These facts point out the removal of matter which is going on in the lagoons and lagoon channels.

Elevation and Subsidence.—Mr Darwin has given many reasons for believing that those islands and coasts which have fringing reefs had recently been elevated, or had long remained in a state of rest.

* A dredging in 155 fathoms, close to the barrier reef of Australia (between it and Raine Island), gave a coral sand, which was, I estimate, more than two-thirds made up of the shells of surface animals.

† Hence in barrier reefs, where the depth outside is very great, we find the reefs running closer to the shore than where the depth is less, and consequently the talus to be formed is smaller.

Throughout the volcanic islands of the great ocean basins the evidence of recent elevations are everywhere conspicuous. Jukes has given most excellent reasons for believing that the coast of Australia fronted by the barrier reef, and even the barrier reef itself, have recently been elevated.* Dana and Couthouy have given a list of islands in almost every barrier reef and atoll region which have recently been elevated.†

This is what we should expect. Generally speaking, all the volcanic regions which we know have in the main been areas of elevation, and we would expect the same to hold good in those vast and permanent hollows of the earth which are occupied by the waters of the ocean. It must be remembered that, probably, all atolls were seated on submarine volcanoes. Areas of local depression are to be looked for in the ocean basins on either side of and between groups of volcanic islands and atolls, and not on the very site of these islands. This is what the deep-sea soundings show if they show any depression at all. Subsidence has been called in in order to account for the existence of lagoons and lagoon channels, and the narrow bands of reef which enclose these; but it has been shown that these were produced by quite other causes,—by the vigorous growth of the corals where most nourishment was to be had, and their death solution and disintegration by the action of sea-water and currents ‡ at those parts which cannot be, on account of their situation, sufficiently supplied with food.

All the chief and characteristic features of barrier reefs and atolls may, indeed, exist with slow elevation, for the removal of lime from the lagoons and the dead upper surface of the reefs by currents, and in solution by rain and sea-water might keep pace with the upward movement.

The most recent charts of all coral reef regions have been examined, and it is found possible to explain all the phenomena by the principles here advanced; while on the subsidence theory, it is most difficult to explain the appearances and structures met with in

* Voyage of the Fly, vol. i. p. 335.

† Dana's Corals and Coral Islands, p. 345. Couthouy's "Remarks on Coral Formations," Bost. Jour. Nat. Hist. See also Stutchbury, West of England Journal.

‡ Very strong currents run out of the entrances into lagoons and lagoon channels, and when the tow-net was used in these entrances it showed that a large quantity of coral detritus was being carried seawards.

many groups; for instance in the Fiji Islands, where fringing reefs, barrier reefs, and atolls, all occur in close proximity, and where all the other evidence seems to point to elevation, or at least a long period of rest. In instances like the Gambier group, the reefs situated on the seaward side of the outer islands would grow more vigorously than those towards the interior; they would extend in the direction of the shallower water, and ultimately would form a continuous barrier around the whole group. The distinguishing feature of the views now advanced is that they do away with the great and general subsidences required by Darwin's theory,* and are in harmony with Dana's views of the great antiquity and permanence of the great ocean basin, which all recent deep-sea researches appear to support.

Summary.—It was shown (1) that foundations have been prepared for barrier reefs and atolls by the disintegration of volcanic islands, and by the building up of submarine volcanoes by the deposition on their summits of organic and other sediments.

(2.) That the chief food of the corals consists of the abundant Pelagic life of the tropical regions, and the extensive solvent action of sea-water is shown by the removal of the carbonate of lime-shells of these surface organisms from all the greater depths of the ocean.

(3.) That when coral plantations build up from submarine banks they assume an atoll form, owing to the more abundant supply of food to the outer margins, and the removal of dead coral rock from the interior portions by currents and by the action of the carbonic acid dissolved in sea-water.

(4.) That barrier reefs have built out from the shore on a foundation of volcanic debris or on a talus of coral blocks, coral sediment, and Pelagic shells, and the lagoon channel is formed in the same way as a lagoon.

(5.) That it is not necessary to call in subsidence to explain any of the characteristic features of barrier reefs or atolls, and that all these features would exist alike in areas of slow elevation, of rest, or of slow subsidence.

In conclusion it was pointed out that all the causes here appealed

* "We may conclude that immense areas have subsided, to an amount sufficient to bury not only any formerly existing lofty table-land, but even the heights formed by fractured strata and erupted matter."—"Coral Reefs," p. 190.

to for an explanation of the structure of coral reefs are proximate, relatively well known, and continuous in their action.

The author expressed his indebtedness to all his colleagues, to Professor Geikie, to the Hydrographer and officers of the hydrographic department, and in a special manner to Sir Wyville Thomson, under whose direction and advice all the observations had been conducted.

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Major-General Bayly, R.E.; Mr W. J. Sollas, M.A.; and Mr Henry Drummond, F.G.S.

Monday, 19*th April* 1880.

SIR WYVILLE THOMSON, Vice-President, in the Chair.

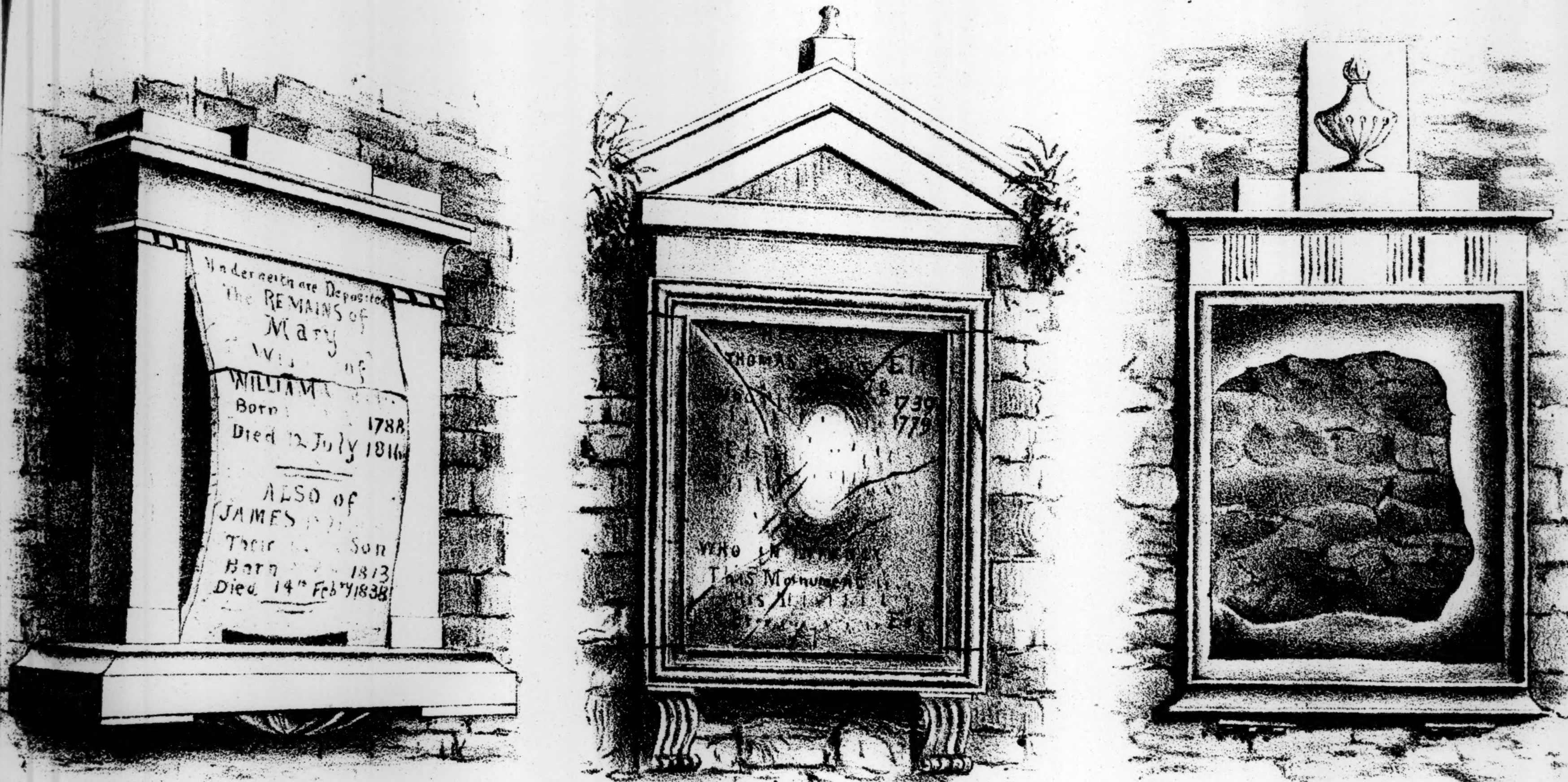
The following Communications were read:—

1. Rock-Weathering, as illustrated in Edinburgh Churchyards. By Professor Geikie, F.R.S. (Plate XVI.)

Comparatively little has yet been done in the way of precise measurement of the rate at which the exposed surfaces of different kinds of rock are removed in the processes of weathering. A few years ago, some experiments were instituted by Professor Pfaff of Erlangen to obtain more definite information on this subject. He exposed to ordinary atmospheric influences carefully measured and weighed pieces of Solenhofen limestone, syenite, granite (both rough and polished), and bone. At the end of three years he found that the loss from the limestone was equivalent to the removal of a uniform layer 0·04 mm. in thickness from its general surface. The stone had become quite dull and earthy, while on parts of its surface fine cracks and incipient exfoliation had appeared.* The time during which the observations were continued was, however, too brief to allow any general deductions to be drawn from them as to the real average rate of disintegration. Professor Pfaff relates that during the period a severe hail storm broke one of the plates of stone. An exceptionally powerful cause of this nature might make

* Allgemeine Geologie als exacte Wissenschaft, p. 317.

To illustrate Prof. Geikie's Paper on Rock-Weathering



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the loss during a short interval considerably greater than the true average of a longer period.

It occurred to me recently that data of at least a provisional value might be obtained from an examination of tombstones freely exposed to the air in graveyards, in cases where their dates remained still legible or might be otherwise ascertained. I have accordingly paid attention to the older burial-grounds in Edinburgh, and have gathered together some facts which have, perhaps, sufficient interest and novelty to be communicated to the Society.

At the outset it is of course obvious that in seeking for data bearing on the general question of rock-weathering, we must admit the kind and amount of such weathering visible in a town to be in some measure different from what is normal in nature. So far as the disintegration of rock-surfaces is effected by mineral acids, for example, there must be a good deal more of such chemical change where sulphuric acid is copiously evolved into the atmosphere from thousands of chimneys than in the pure air of country districts. In these respects we may regard the disintegration in towns as an exaggeration of the normal rate. Still, the difference between town and country may be less than might be supposed. Surfaces of stone are apt to get begrimed with dust and smoke, and the crust of organic and inorganic matter deposited upon them may in no small measure protect them from the greater chemical activity of the more acid town rain. In regard to the effect of daily or seasonal changes of temperature, on the other hand, any difference between town and country may not impossibly be on the side of the town. Owing, probably, to the influence of smoke in retarding radiation, thermometers placed in open spaces in town commonly mark an extreme nocturnal temperature not quite so low as those similarly placed in the suburbs, while they show a maximum day temperature not quite so high.

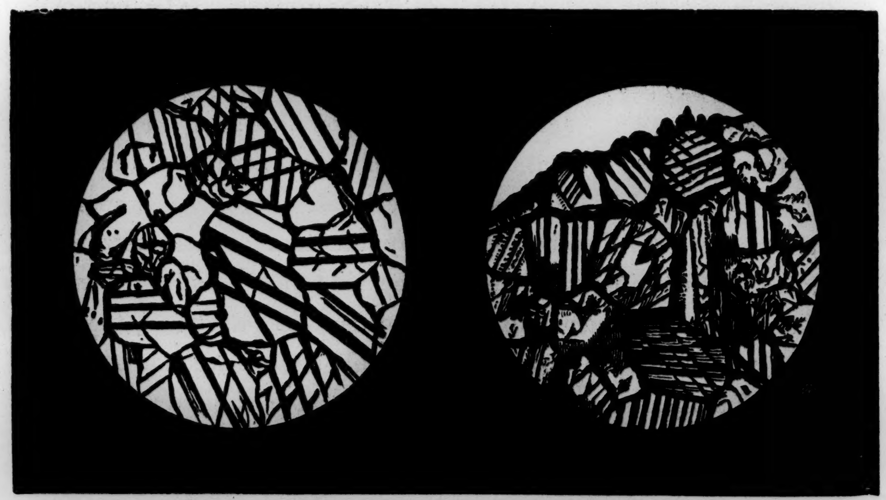
The illustrations of rock-weathering presented by city graveyards are necessarily limited to the few kinds of rock employed for monumental purposes. In this district the materials used are of three kinds:—1st, Calcareous, including marbles and limestones; 2d, sandstones and flagstones; 3d, granites.

I. Calcareous.—With extremely rare exceptions, the calcareous tombstones in our graveyards are constructed of ordinary white

saccharoid Italian marble. I have also observed a pink Italian shell-marble, and a finely fossiliferous limestone, containing fragments of shells, foraminifera, &c.

In a few cases the white marble has been employed by itself as a monolith in the shape of an obelisk, urn, or other device; but most commonly it occurs in slabs which have been tightly fixed in a framework of sandstone. These slabs, from less than 1 to fully 2 inches thick, are generally placed vertically; in one or two examples they have been inserted in large horizontal sandstone slabs or "through-stanes." The form into which it has been cut, and the position in which it has been erected, have had considerable influence on the weathering of the stone.

A specimen of the common white marble employed for monumental purposes was obtained from one of the marble-works of the city, and examined microscopically. It presented the well-known granular character of true saccharoid marble, consisting of rounded granules of clear transparent calcite, averaging about $\frac{1}{100}$th of an inch in diameter (fig. 1, A). Each granule has its own system of



A B

Fig. 1.—Microscopic structure of white marble employed in Edinburgh tombstones. A, Structure of the fresh marble. B, Structure of the marble after standing eighty-seven years. The black edge is the crust of sulphate of lime and town dust which descends along rifts and cleavage planes.

twin lamellations, and not infrequently gives interference colours. The fundamental rhombohedral cleavage is everywhere well de-

veloped. Not a trace exists of any amorphous granular matrix or base holding the crystalline grains together. These seem moulded into each other, but have evidently no extraordinary cohesion. A small fragment placed in dilute acid was entirely dissolved. There can be no doubt that this marble must be very nearly pure carbonate of lime.

The process of weathering in the case of this white marble presents three phases sometimes to be observed on the same slab,—viz., superficial solution, internal disintegration, and curvature with fracture.

(1.) *Superficial Solution* is effected by the carbonic acid, and partly by the sulphuric acid of town-rain. When the marble is first erected it possesses a well-polished surface, capable of affording a distinct reflection of objects placed in front of it. Exposure for not more than a year or two to our prevalent westerly rains suffices to remove this polish, and to give the surface a rough granular character. The granules which have been cut across or bruised in the cutting and polishing process are first attacked and removed in solution, or drop out of the stone. An obelisk in Greyfriars' Churchyard, erected in memory of a lady who died in 1864, has so rough and granular a surface that it might readily be taken for a sandstone. So loosely are the grains held together that a slight motion of the finger will rub them off. In the course of solution and removal, the internal structure of the marble begins to reveal itself. Its harder nests and veinings of calcite and other minerals project above the surrounding surface, and may be traced as prominent ribs and excrescences running across the faint or illegible inscriptions. On the other hand, some portions of the marble are more rapidly removed than others. Irregular channels, dependent partly on the direction given to trickling rain by the form of the monumental carving, but chiefly on original differences in the internal structure of the stone, are gradually hollowed out. In this way the former artificial surface of the marble disappears, and is changed into one that rather recalls the bare bleached rocks of some mountain side.

The rate at which the transformation takes place seems to depend primarily on the extent to which the marble is exposed to rain. Slabs which have been placed facing to north-east, and with a sufficiently projecting architrave to keep off much of the rainfall, retain

their inscriptions legible for a century or longer. But even in these cases the progress of internal disintegration is distinctly visible. Where the marble has been less screened from rain, the rapidity of waste has been sometimes very marked. A good illustration is supplied by the tablet on the south side of Greyfriars' Churchyard, erected in memory of G—— G——, who died in 1785.* This monument had become so far decayed as to require restoration in 1803. It is now and has been for some years for the most part utterly illegible. The marble has been dissolved away over the centre of the slab to a depth of about a quarter of an inch. Yet this monument is by no means in an exposed situation. It faces eastward in a rather sheltered corner, where, however, the wind eddies in such a way as to throw the rain against the part of the stone which has been most corroded.

In the majority of cases superficial solution has been retarded by the formation of a peculiar grey or begrimed crust, to be immediately described. The marble employed here for monumental slabs appears to be peculiarly liable to the development of this crust. Another kind of white marble, sometimes employed for sculptured ornaments on tombstones, dissolves without crust. It is snowy white, and more translucent than the ordinary marble. So far as the few weathered specimens I have seen enable me to judge, it appears to be either Carrara marble, or one of the strongly saccharoid, somewhat translucent, varieties employed instead of it. This stone, however, though it forms no crust, suffers marked superficial solution. But it escapes the internal disintegration, which, so far as I have observed, is always an accompaniment of the crust. Yet the few examples of it I have met with hardly suffice for any comparison between the varieties.

(2.) *Internal Disintegration.*—Many of the marble monuments in our older churchyards are covered with a dirty crust, beneath which the stone is found on examination to be merely a loose crumbling sand. This crust seems to form chiefly where superficial solution is feeble. It may be observed to crack into a polygonal network, the individual polygons occasionally curling up so as to reveal the yellowish white crumbling material underneath. It also rises in

* For obvious reasons I withhold the names carved on the tombstones referred to in this communication.

blisters which, when they break, expose the interior to rapid disintegration.

So long as this begrimed film lasts unbroken, the smooth face of the marble slab remains with apparently little modification. The inscription may be perfectly legible. The moment the crust is broken up, however, the decay of the stone is rapid. For we then see that the cohesion of the individual crystalline granules of the marble has already been destroyed, and that the merest touch causes them to crumble into a loose sand.

It appears, therefore, that two changes take place in upright marble slabs freely exposed to rain in our burial-grounds—a superficial, more or less firm crust is formed, and the cohesion of the particles beneath is destroyed.

The crust varies in colour from a dirty grey to a deep brown black, and in thickness from that of writing paper up to sometimes at least a millimetre. One of the most characteristic examples of it was obtained from an utterly decayed tomb (erected in the year 1792) on the east side of Canongate Churchyard. No one would suppose that the pieces of flat dark stone lying there on the sandstone plinth were once portions of white marble. Yet a mere touch suffices to break the black crust, and the stone at once crumbles to powder. Nevertheless the two opposite faces of the original polished slab have been preserved, and I even found the sharply chiselled socket-hole of one of the retaining nails. The specimen was carefully removed, and soaked in a solution of gum, so as to preserve it from disintegration. On submitting the crust of this marble to microscopic investigation I found it to consist of particles of coal, grains of quartz-sand, angular pieces of broken glass, fragments of red brick or tile, and organic fibres. This miscellaneous collection of town dust was held together by some amorphous cement, which was not dissolved by hydrochloric acid. At my request my friend Mr B. N. Peach tested it with soda on charcoal, and at once obtained a strong sulphur reaction. There can be little doubt that it is mainly sulphate of lime. The crust which forms upon our marble tombstones is thus a product of the reaction of the sulphuric acid of the town-rain upon the carbonate of lime. A pellicle of amorphous gypsum is deposited upon the marble, and encloses the particles of dust which give the characteristic sooty aspect to the stone. This pellicle, of

course, when once formed, is comparatively little affected by the chemical activity of rain-water. Hence the conservation of the even surface of the marble. It is liable, however, to be cracked by an internal expansion of the stone, to which I shall immediately refer, and also to rise in small blisters, and, as I have said, its rupture leads at once to the rapid disintregation of the stone.

The cause of this disintegration is the next point for consideration. Chemical examination revealed the presence of a slight amount of sulphate in the heart of the crumbling marble; but the quantity appeared to me to be too small seriously to affect the cohesion of the stone. I submitted to microscopic examination a portion of a crumbling urn of white marble in Canongate Churchyard. The tomb bears a perfectly fresh date of 1792 cut in sandstone over the top; but the marble portions are crumbling into sand, though the structure faces the east, and is protected from vertical rain by arching mason-work. A small portion of the marble retaining its crust was boiled in Canada balsam, and was then sliced at a right angle to its original polished surface. By this means a section of the crumbled marble was obtained, which could be compared with one of the perfectly fresh stone (see fig. B). From the dark outer amorphous crust, with its carbonaceous and other miscellaneous particles, fine rifts could be seen passing down between the separated calcite granules, which in many cases were quite isolated. The black crust descends into these rifts, and likewise passes along the cleavage planes of the granules. Towards the outer surface of the stone, immediately beneath the crust, the fissures are chiefly filled with a yellowish structureless substance, which gave a feeble glimmering reaction with polarised light, and enclosed minute amorphous aggregates like portions of the crust. It probably consists chiefly of sulphate of lime. But the most remarkable feature in the slide was the way in which the calcite granules had been corroded. Seen with reflected light they resembled those surfaces of spar which have been placed in weak hydrochloric acid to lay bare enclosed crystals of zeolites. The solution had taken place partly along the outer surfaces, so as to produce the fine passages or rifts, and partly along the cleavage. Deep cavities, defined by intersecting cleavage planes, appeared to descend into the heart of some of the granules. In no case did I observe any white pellicle such as might indicate a re-deposit of

lime from the dissolved carbonate. Except for the veinings of probable sulphate just referred to, the lime, when once dissolved, had apparently been wholly removed in solution. There was further to be observed a certain dirtiness, so to speak, which at the first glance distinguished the section of crumbled marble from the fresh stone. This was due partly to corrosion, but chiefly to the introduction of particles of soot and dust, which could be traced among the interstices and cleavage lamellæ of the crystalline granules for some distance back from the crust.

It may be inferred, therefore, that the disintegration of the marble is mainly due to the action of carbonic acid in the permeating rain-water, whereby the component crystalline granules of the stone are partially dissolved and their mutual adhesion is destroyed. This process goes on in all exposures and with every variety in the thickness of the outer crust. It is distinctly traceable in tombstones that have not been erected for more than twenty years. In these which have been standing for a century it is, save in exceptionally sheltered positions, so far advanced that a very slight pressure suffices to crumble the stone into powder. But with this internal disintegration we have to take into consideration the third phase of weathering to which I have alluded. In the upright marble slabs it is the union of the two kinds of decay which leads to so rapid an effacement of the monuments.

(3.) *Curvature and Fracture.*—This most remarkable phase of rock-weathering is only to be observed in the slabs of marble which have been firmly inserted into a solid framework of sandstone, and placed in an erect or horizontal position. It consists in the bulging out of the marble accompanied with a series of fractures. This change cannot be explained as mere sagging by gravitation, for it usually appears as a swelling up of the centre of the slab, which continues until the large blister-like expansion is disrupted. Nor is it by any means exceptional; it occurs, as a rule, on all the older upright marble tablets, and is only found to be wanting in those cases where the marble has evidently not been fitted tightly into its sandstone frame. Wherever there has been little or no room for expansion, protuberance of the marble may be observed. Successive stages may be seen from the first gentle uprise to an unsightly swelling of the whole stone. This change is accompanied by fracture of the

marble. The rents in some cases proceed from the margin inwards, more particularly from the upper and under edges of the stone, pointing unmistakably to an increase in volume as the cause of fracture. In other cases the rents appear in the central part of the swelling where the tension from curvature has been greatest.

Some exceedingly interesting examples of this singular process of weathering are to be seen in Greyfriars' Churchyard. On the south wall, in the enclosure of a well-known county family, there is an oblong upright marble slab (Pl. XVI., A, measuring $30\frac{1}{4}$ inches in height, by $22\frac{3}{8}$ inches in breadth and $\frac{3}{4}$ inch in thickness, and facing west. The last inscription on it bears the date 1838, at which time, of course, it was no doubt still smooth and upright. Since then, however, it has escaped from its fastenings on either side, though still held firmly at the top and bottom. It consequently projects from the wall like a well-filled sail. The axis of curvature is, of course, parallel to the upper and lower margins, and the amount of deviation from the original vertical line is fully $2\frac{1}{2}$ inches, so that the hand and arm can be inserted between the curved marble and the perfectly vertical and undisturbed wall to which it was fixed. At the lower end of the slab a minor curvature to the extent of $\frac{1}{8}$th of an inch is observable, coincident with the longer axis of the stone. In both cases the direction of the bending has been determined by the position of the enclosing solid frame of sandstone which resisted the internal expansion of the marble. Freed from its fastenings at either side the stone had assumed a simple wave-like curve. But the tension has become so great that a series of rents has appeared along the crest of the fold. One of these has a breadth of $\frac{1}{10}$th of an inch at its opening.* Not only has the slab been ruptured, but its crust has likewise yielded to the strain, and has broken up into a network of cracks, and some of the isolated portions are beginning to curl up at the edges, exposing the crumbling decayed marble below. I should add that such has been the expansive force of the marble that the part of the sandstone block in the upper part of the frame, exposed to the direct pressure, has begun to exfoliate, though elsewhere the stone is quite sound.

* It is a further curious fact that the slab measures $\frac{1}{2}$ inch more in breadth across the centre, where it has had room to expand, than at the top, where it has been tightly jammed between the sandstone slabs.

More advanced stages of curvature and fracture may be noticed on many other tombstones in the same burying-place. One of the most conspicuous of these has a peculiar interest from the fact that it occurs on the tablet erected to the memory of one of the most illustrious dead whose dust lies within the precincts of the Greyfriars—the great Joseph Black. He died in 1799. In the centre of the sumptuous tomb raised over his grave is inserted a large upright slab of white marble, which, facing south, is protected from the weather partly by heavy overhanging masonry and partly by a high stone wall immediately to the west. On this slab a Latin inscription records with pious reverence the genius and achievements of the discoverer of carbonic acid and latent heat; and adds, that his friends wished to mark his resting-place by the marble whilst it should last. Less than eighty years, however, have sufficed to render the inscription already partly illegible. The stone, still firmly held all round its margin, has bulged out considerably in the centre, and on the blister-like expansion has been rent by numerous cracks which run, on the whole, in the direction of the length of the stone.

A further stage of decay is exhibited by a remarkable tomb on the west wall of the Greyfriars' Churchyard (Pl. XVI., B). The marble slab, bearing a now almost wholly effaced inscription, on which the date 1779 can be seen, is still held tightly within its enclosing frame of sandstone slabs, which are firmly built into the wall. But it has swollen out into a ghastly protuberance in the centre, and is, moreover, seamed with rents which strike inwards from the margins. In this and in some other examples the marble seems to have undergone most change on the top of the swelling, partly from the system of fine fissures by which it is broken up, and partly from more direct and effective access of rain. Eventually the cohesion of the stone at that part is destroyed, and the crumbling marble falls out, leaving a hole in the middle of the slab. When this takes place disintegration proceeds rapidly. Three years ago I sketched a tomb in this stage on the east wall of Canongate Churchyard (Pl. XVI., C). In a recent visit to the place I found that the whole of the marble had since fallen out.

The first cause that naturally suggests itself in explanation of the remarkable change in the structure of a substance, usually regarded as so inelastic, is the action of frost. White statuary marble is

naturally porous. It is rendered still more so by that internal solution which I have described. The marble tombstones in our graveyards are, therefore, capable of imbibing a relatively large amount of moisture. When this interstitial water is frozen, its expansive force, as it passes into the solid state, must increase the isolation of the granules and augment the dimensions of a marble block. I am inclined to believe that this must be the principal cause of the change. Whatever may be the nature of the process it is evidently one which acts from within the marble itself. Microscopic examination fails to discover any chemical transformation which would account for the expansion. Dr Angus Smith has pointed out that in towns the mortar of walls may be observed to swell up and lose cohesion from a conversion of its lime into the condition of sulphate. I have already mentioned that sulphate does exist within the substance of the marble, but that its quantity, so far as I have observed, is too small to be taken into account in this question. The expansive power is exerted in such a way as not sensibly to affect the internal structure and composition of the stone. And this I imagine is most probably the work of frost.

The results of my observations among our burial-grounds show that, save in exceptionally sheltered situations, slabs of marble, exposed to the weather in such a climate and atmosphere as that of Edinburgh, are entirely destroyed in less than a century. Where this destruction takes place by simple comparatively rapid superficial solution and removal of the stone, the rate of lowering of the surface amounts sometimes to about a third of an inch (or roughly 9 millimetres) in a century. Where it is effected by internal displacement, a curvature of 2½ inches, with abundant rents, a partial effacement of the inscription, and a reduction of the marble to a pulverulent condition, may be produced in about forty years, and a total disruption and effacement of the stone within one hundred. It is evident that white marble is here utterly unsuited for out of door use, and that its employment for really fine works of art which are meant to stand in the open air in such a climate ought to be strenuously resisted. Of course I am now referring, not to the durability of marble generally, but to its behaviour in a large town with a moist climate and plenty of coal-smoke.

II. Sandstones and Flagstones.—These, being the common

building materials of the country, are of most frequent occurrence as monumental stones. Where properly selected they are remarkably durable. By far the best varieties are those which consist of a nearly pure fine siliceous sand, with little or no iron or lime, and without trace of bedding structure. Some of our sandstones contain 98 per cent. of silica. A good illustration of their power of resisting the weather is supplied by Alexander Henderson's tomb in Greyfriars' Churchyard. He died in 1646, and a few years afterwards the present tombstone, in the form of a solid square block of freestone, was erected at his grave. It was ordered to be defaced in 1662 by command of the Scottish Parliament, but after 1688 it was repaired. Certain bullet marks upon the stone are pointed out as those of the soldiery sent to execute the order. Be this as it may, the original chisel marks on the polished surface of the stone are still perfectly distinct, and the inscribed lettering remains quite sharp. Two hundred years have effected hardly any change upon the stone, save that on the west and north sides, which are those most exposed to wind and rain, the surface is somewhat roughened, and the internal fine parallel jointing begins to show itself.

Three obvious causes of decay in arenaceous rocks may be traced among our monuments. In the first place, the presence of a soluble or easily removable matrix in which the sand grains are embedded. The most common kinds of matrix are clay, carbonates of lime and iron, and the anhydrous and hydrous peroxides of iron. The presence of the iron reveals itself by its yellow, brown, or red colour. So rapid is disintegration from this cause that the sharply incised date of a monument erected in Greyfriars' Church to an officer who died only in 1863 is no longer legible. At least $\frac{1}{8}$th of an inch of surface has here been removed from a portion of the slab in 16 years, or at the rate of about three quarters of an inch in a century.

In the second place, where a sandstone is marked by distinct laminæ of stratification, it is nearly certain to split up along these lines under the action of the weather, if the surface of the bedding planes is directly exposed. This is well known to builders, who are quite aware of the importance of "laying a stone on its bed." Examples may be observed in our churchyards where sandstones of this character have been used for pilasters and ornamental work and

where the stone, set on its edge, has peeled off in successive layers. In flagstones, which are merely thinly bedded sandstones, this minute lamination is often fatal to durability. These stones, from the large size in which slabs of them can be obtained, and from the ease with which they can be worked, form a tempting material for monumental inscriptions. The melancholy result of trusting to their permanence is strikingly shown by a tombstone at the end of the south burying-ground in Greyfriars' Churchyard. The date inscribed on it is 1841, and the lettering that remains is as sharp as if cut only recently. The stone weathers very little by surface disintegration. It is a laminated flagstone set on edge, and large portions have scaled off, leaving a rough, raw surface where the inscription once ran. In this instance a thickness of about $\frac{1}{3}$rd of an inch has been removed in forty years.

In the third place, where a sandstone contains concretionary masses of different composition or texture from the main portion of the stone, these are apt to weather at a different rate. Sometimes they resist destruction better than the surrounding sandstone so as to be left as permanent excrescences. More commonly they present less resistance, and are therefore hollowed out into irregular and often exceedingly fantastic shapes. Examples of this kind of weathering abound in our neighbourhood. Perhaps the most curious to which a date can be assigned are to be found in the two sandstone pillars, which until recently flanked the tomb of Principal Carstares in Greyfriars' Churchyard. They were erected some time after the year 1715. Each of them is formed of a single block of stone about 8 feet long. Exposure to the air for about 150 years has allowed the original differences of texture or composition to make their influence apparent. Each column is hollowed out for almost its entire length on the exposed side into a trough 4 to 6 inches deep and 6 to 8 inches broad. As they lean against the wall, beneath the new pillars which have supplanted them, they suggest some rude form of canoe rather than portions of a sepulchral monument.

Where concretions are of a pyritous kind their decomposition gives rise to sulphuric acid, some of which combines with the iron and gives rise to dark stains upon the corroded surface of the stone. Some of the sandstones of the district, full of such impurities, ought never to be employed for architectural purposes. Every

block of stone in which they occur should be unhesitatingly condemned. Want of attention to this obvious rule has led to the unsightly disfigurement of public buildings.

III. Granites.—In Professor Pfaff's experiments, to which I have already referred, he employed plates of syenite and granite, both rough and polished. He found that they had all lost slightly in weight at the end of a year. The annual rate of loss was estimated by him as equal to 0·0076 mm. from the unpolished, and 0·0085 from the polished granite. That a polished surface of granite should weather more rapidly than a rough one is perhaps hardly what might have been expected. The same observer remarks, that though the polished surface of syenite was still bright at the end of not more that three years, it was less so than at first; and in particular, that some figures indicating the date, which he had written on it with a diamond, had become entirely effaced. Granite has been employed for too short a time as a monumental stone in our cemeteries to afford any ready means of measuring even appproximately its rate of weathering. Traces of decay in some of its felspar crystals may be detected, yet in no case that I have seen is the decay of a polished granite surface sensibly apparent after exposure for fifteen or twenty years. That the polish will disappear, and that the surface will gradually roughen as the individual component crystals are more or less easily attacked by the weather, is of course sufficiently evident. Even the most durable granite will probably be far surpassed in permanence by the best of our siliceous sandstones. But as yet the data do not exist for making any satisfactory comparison between them.

[Note added 21st May 1880. Since the preceding paper was written, I have had an opportunity of examining the condition of the monumental stones in the graveyards of a number of towns and villages in the north-east of Scotland, where the population is sparse and where comparatively little coal-smoke passes into the atmosphere. The marble tablets last longer there than in Edinburgh, but show everywhere indications of decay. They appear to be quite free from the black or grey sulphate-crust. They suffer chiefly from superficial erosion, but I observed a few cases of curvature and and fracture. As a contrast to the universal decay of the marble tombstones, reference may be made to the remarkable durability of the clay-slate which has been employed for monumental purposes

in Aberdeenshire. It is a fine-grained, rather soft rock, containing scattered cubes of pyrites, and capable of being readily dressed into thin smooth slabs. A tombstone of this material, erected in the old burying-ground at Peterhead, sometime between 1785 and 1790, retains its lettering as sharp and smooth as if only recently incised. Yet the stone is soft enough to be easily cut with the knife. The cubes of pyrites have resisted weathering so well, that a mere thin film of brown hydrous peroxide conceals the brassy undecomposed sulphide from view. The slate is slightly stained yellow round each cube or kernel of pyrites, but its general smooth surface is not affected. The lapse of nearly a century has produced scarcely any change upon this stone, while neighbouring tablets of white marble, 100 to 150 years old, present rough granular surfaces and half-effaced though still legible inscriptions.]

2. On a Realised Sulphurous Acid Steam-Pressure Thermometer, and on a Sulphurous Acid Steam-Pressure Differential Thermometer. By Sir William Thomson.

A sulphurous acid steam-pressure thermometer, on the plan described in my communication on the subject to the Royal Society of March 1, has been actually constructed, with range up to 25° C., but not yet in a permanent form. The slight trials I have been able to make with it give promise that, in respect to sensibility and convenience for practical use, it will most satisfactorily fulfil all expectations, and have given some experience in respect to the overcoming of difficulties of construction, from which the following instructions are suggested as likely to be useful to any one who may desire to make such an instrument:—

(1.) The sulphurous acid steam thermometer might more properly be called a cryometer than a thermometer, because it is not very convenient, except for measuring temperatures lower than the atmospheric temperature at the place and time of observation; for, it must be remarked, that the thermometric substance, that is to say, the infinitesimal layer of liquid and steam of sulphurous acid at the interface between the two in the bulb in the annexed drawing (fig. 1), must be at a lower temperature than any other part of the space of bulb and tube between it and the mercury

surface in the shorter vertical column. It is satisfactory, however, that the instrument is really not needed for temperatures above $+10°$ C., because for such the water steam-pressure thermometer, represented in the first of the three diagrams of my former communication, has ample sensibility for most practical purposes. Hence, instead of the range up to 25° C. in the instrument already realised, and the great length of tube (295 centimetres for the long vertical branch) which it requires, I propose in future to let $+10°$ be the superior limit of the temperatures to be measured by an ordinary sulphurous acid steam thermometer. For this, the long vertical branch need not be more than 175 centimetres; thus the instrument is much more easily made, and when made, is much less cumbrous.

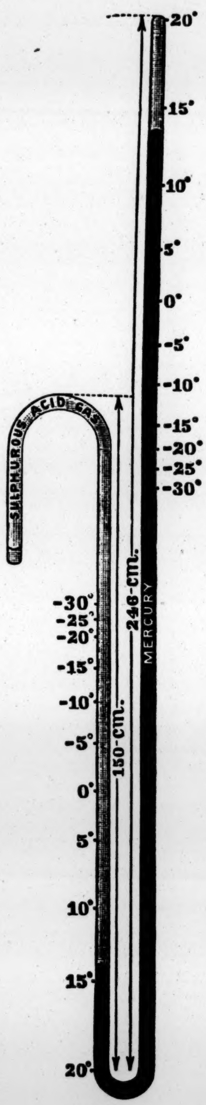


Fig. 1.

(2.) The upper end of the long branch, being open to begin with, is to be securely cemented to a small and very perfectly air-tight iron stopcock L, communicating with an iron pipe, bent at right angles, as shown in the drawing (fig. 2). This iron pipe is, in the first place, to be put into communication temporarily by an india-rubber junction with the generator, and with an air-pump, by means of a metal branch tube, with two stopcocks R and S, as shown in the drawing.

(3.) To begin, close R and open S and L; and exhaust moderately (down to half an inch of mercury will suffice). Warm the whole length of the bent tube moderately by a spirit lamp, or spirit lamps, to dry the inner surface sufficiently. Then, still maintaining the exhaustion by the air-pump, apply a freezing mixture to the bulb and shorter vertical tube, and all of the long vertical tube except a convenient length of a foot or two next its upper end, as shown in the drawing. Before joining the generator to Q, let enough of sulphurous acid gas be passed out through P to clear out fairly well the air from the generator and the purifying sulphuric acid wash-

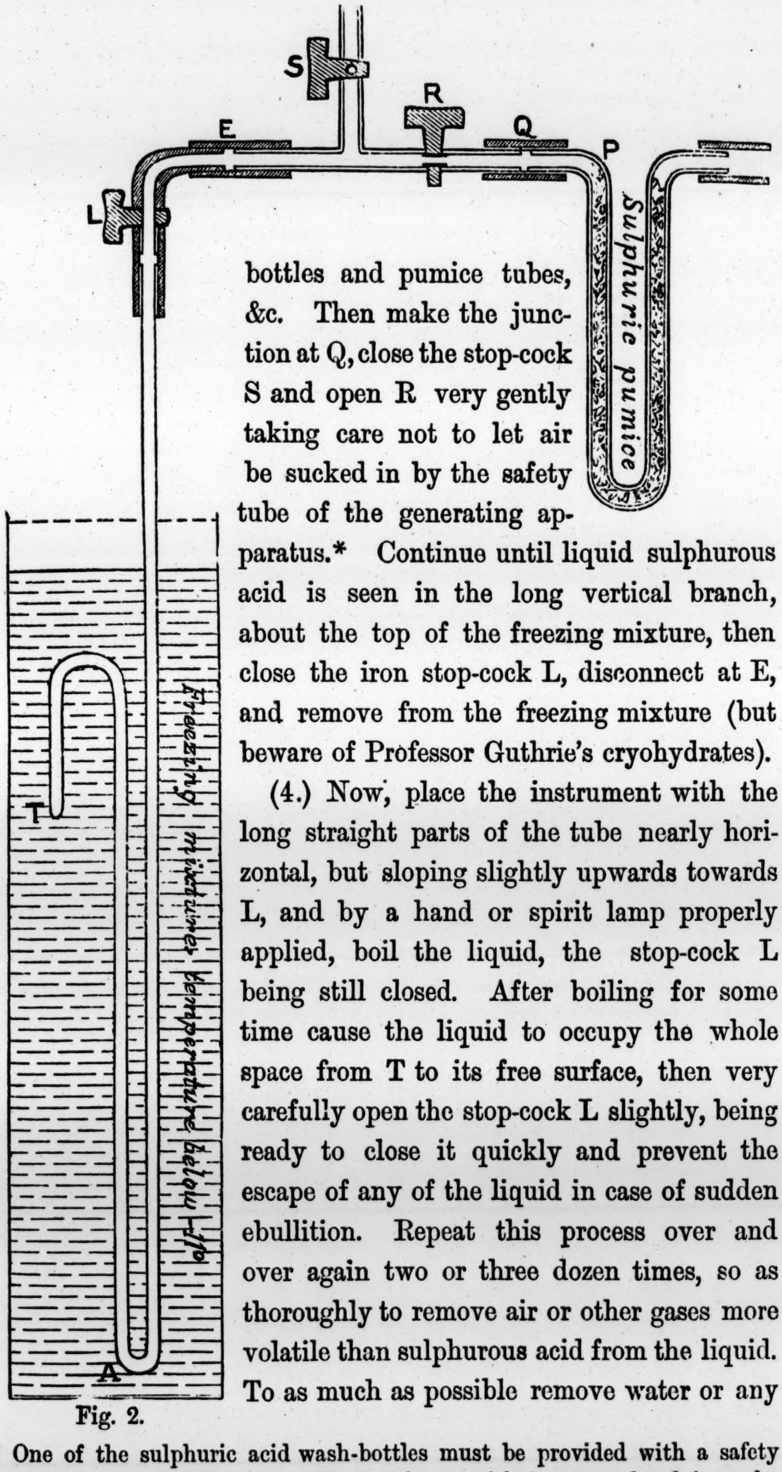


Fig. 2.

bottles and pumice tubes, &c. Then make the junction at Q, close the stop-cock S and open R very gently taking care not to let air be sucked in by the safety tube of the generating apparatus.* Continue until liquid sulphurous acid is seen in the long vertical branch, about the top of the freezing mixture, then close the iron stop-cock L, disconnect at E, and remove from the freezing mixture (but beware of Professor Guthrie's cryohydrates).

(4.) Now, place the instrument with the long straight parts of the tube nearly horizontal, but sloping slightly upwards towards L, and by a hand or spirit lamp properly applied, boil the liquid, the stop-cock L being still closed. After boiling for some time cause the liquid to occupy the whole space from T to its free surface, then very carefully open the stop-cock L slightly, being ready to close it quickly and prevent the escape of any of the liquid in case of sudden ebullition. Repeat this process over and over again two or three dozen times, so as thoroughly to remove air or other gases more volatile than sulphurous acid from the liquid. To as much as possible remove water or any

* One of the sulphuric acid wash-bottles must be provided with a safety tube with overflow bulb. An ordinary pipette with its stem fitted into the india-rubber stopper of the bottle will serve for the purpose.

fluid less volatile than sulphurous acid, proceed as follows:—Apply heat at T and in the bend next T until the liquid leaves that part of the enclosure and stands nearly at a level in the short and long vertical branch, the instrument being held with A down. Apply a freezing mixture to T, taking care not to cool it to quite as low a temperature as $-11°$ C.; so that the pressure of the sulphurous acid liquid and steam may remain something above the external atmospheric pressure. Occasionally open the stop-cock L very slightly to prevent the liquid from being drawn up the short vertical branch through preponderance of temperature in the long vertical branch. Continue this until about a centimetre of liquid has been distilled over into the bulb T. Then open the stop-cock L very carefully until all the liquid in the two vertical branches is blown out, leaving that which has been distilled over into the bulb T, and then close L again.

(6.) Then dip the end E under pure mercury, and by opening L very gently and warming the free surface of the liquid sulphurous acid, let gas escape bubbling up through the mercury. Close L again before or when the quantity of liquid in the bulb at T begins to be perceptibly diminished. Then apply a freezing mixture to T until mercury is drawn in. Incline the instrument with A up and L down, and watch until the mercury is drawn up to A, then incline with A down and let a little more mercury come in. Then close L. Lastly, keeping T still in the freezing mixture, melt the glass below L till it collapses and blows the mercury down, leaving Torricellian vacuum at the sealed end. The instrument is now complete and ready for use.

Sulphurous Acid Steam-Pressure Differential Thermometer.

This consists of a U tube, with its ends bent down, as shown in the drawing, containing mercury in the main bend and in the lower parts of the straight vertical branches, and sulphurous acid gas, steam, and liquid in the rest of the enclosure. Every other part of the enclosure must be kept somewhat warmer than the warmer of the two ends, T, T′.

The infinitesimal quantities of matter in the transitional layers, between liquid and steam, at T and T′, constitute the thermometric

substance. The gas between these and the manometric mercury, and the mercury serve merely the purpose of transmitting the steam-pressures, and measuring the difference between them.

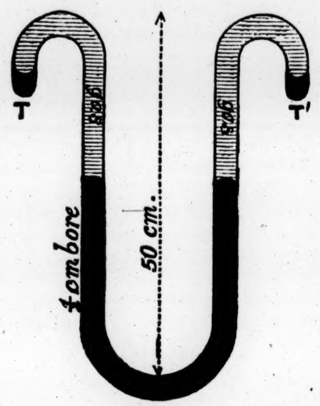


Fig. 3.

At $12\frac{1}{2}°$ C., the sensibility of the instrument, as we see by Regnault's tables of sulphurous acid steam-pressures, quoted in the article "Heat," of the eleventh volume of the *Encyclopædia Britannica*, is 7 centimetres difference of mercury levels, to 1° difference of temperature (that is temperatures 12°, 13°) at T, T′ respectively.

At $22\frac{1}{2}°$ the sensibility similarly reckoned, is 12·2 cms. to 1°.

Note on Steam-Pressure Thermometers.
By Sir William Thomson.

If the bore of the vertical tube is less than three or four millimetres, there ought to be an enlargement at its upper end, or else there should not be quite enough of the liquid to fill the tall manometric tube; otherwise, if in the use of the instrument the liquid is pressed up to the top of the vertical tube, it is impossible to get it down again except by the tedious operation of distilling the whole liquid from the tube into the bulb, by applying heat by means of a spirit-lamp or a large vessel of hot water to the manometric tube, which, to facilitate this operation, may be held inclined with the closed end down. An instrument like that shown in fig. 1 of my former communication (March 1, 1880), with vertical tube of the diameter (2 or 3 millimetres) there stated is subject to this inconvenience, although, in my first attempts to realise the instrument, imperfect removal of air from the water and steam in the enclosed space prevented me from experiencing it. The difficulty, of course, might have been foreseen; but I did not think it would have been so great as I now find it to be with an instrument constructed exactly according to fig. 1 of my former communication, with air very perfectly removed from the enclosure by a proper process of boiling before sealing the instrument.

3. On a Differential Thermoscope founded on Change of Viscosity of Water with change of Temperature. By Sir Wm. Thomson.

Water flows from a little cistern or reservoir R through a wide vertical tube S, about two metres long, thence through a horizontal capillary tube C′, 50 or 100 centimetres long; thence through a wide horizontal metal tube T, 20 or 30 centimetres long; thence through a second horizontal capillary C; and lastly, out by a little constant-level overflow cup L. A vertical glass manometric tube M, a metre and a half long, standing up above the end of T next C to measure the pressure in T by a water column; and a means of giving any uniform temperature to the outsides of S and C′, and any other uniform temperature to the outsides of T and C′; complete the instrument.

Denote the heights of the levels of the water in R and M, above L, by h and $\frac{1}{2}h - x$. If C and C′ are equal and similar, or otherwise so proportioned as to be equal in their resistances to the flow of the water at equal temperatures through them, we find from the formula by which Poiseulle expressed the results of his experiments on the flow of water through capillary tubes—

$$x = \tfrac{1}{2}h \frac{\cdot 03368 \,.\, \frac{1}{2}(t - t') + \cdot 000221 \,.\, \frac{1}{2}(t^2 - t'^2)}{1 + \cdot 03368 \,.\, \frac{1}{2}(t + t') + \cdot 000221 \,.\, \frac{1}{2}(t^2 + t'^2)},$$

where t' and t denote the temperatures of the water as it flows through C′ and C. By the arrangements described it is secured that t is very nearly the same as the temperature of the outsides of B and C. Thus, if $h = 200$ cms., $t' = 0°$, and $t = 1°$, we have $x = 3{\cdot}3$. Thus the sensibility is 33 mms. per 1° C.; and 1/30 of a degree would therefore be very perceptible.

Even with its high sensibility this instrument may not be frequently found convenient for thermal researches, and its chief use may be for illustration of Poiseulle's important discovery.

4. On a Thermomagnetic Thermoscope. By Sir W. Thomson.

This thermoscope is founded on the change produced in the magnetic moment of a steel magnet by change of temperature. Several different forms suggest themselves: the one which seems best adapted to give good results is to be made as follows:—

(1.) Prepare an approximately astatic system of two thin, hardened steel wires, $r\ b$, $r'\ b'$, each 1 cm. long, one of them, $r\ b$, hung by a single silk fibre, and the other hung bifilarly from it, by fibres about 3 cms. long, so attached that the projections of the two, on a horizontal plane, shall be inclined at an angle of about ·01 of a radian (or ·57°) to one another.

(2.) Hang a very small light mirror bifilarly from the lower of the two wires.

(3.) Magnetise the two wires to very exactly equal magnetic moments in the dissimilar directions. This is easily done by a few successive trials, to make them rest as nearly as possible perpendicular to the magnetic meridian.

(4.) Take two pieces of equal and similar straight steel wire, well hardened, each 2 cms. long, and about ·04 cm. diameter; magnetise them equally and similarly; and mount them on a suitable frame to fulfil conditions (5) and (6). Call them R B and R′ B′, B and B′ denoting the ends containing true north polarity (ordinarily marked B), and R R′ true south (ordinarily marked red). The small letters r, b, r', b' mark, on the same plan, the polarities of $r\ b$ and $r'\ b'$.

(5.) The magnets R B, R′ B′, are to be relatively fixed in line on their frame, with similar poles next one another, at a distance of about 2 cms. asunder; as thus R B . . . B′ R′, with B B′ = 2 cms.

(6.) This frame is to be mounted on a geometrical slide upon the case within which the astatic pair $r\ b$, $r'\ b'$ is hung, in such a manner that the line of R B, B′ R′ bisects $r\ b$, approximately at right angles, and that R B B′ R′ may be moved by a micrometer screw through about a millimetre on each side of its central position, the line of motion being the line of R B, B′ R′, and the

"central position" being that in which B and B′ are equidistant from the centre of $r\,b$.

(7.) A lamp and scale, with proper focussing lens if the mirror is not concave, are applied to show and measure small deflections as in my mirror galvanometers and electrometer.

Use of the Thermoscope.

(8.) Place the instrument with the needles approximately perpendicular to the magnetic meridian, turning it so as to bring b and b' to the south side of the vertical plane bisecting the small angle between the projections of $r\,b$, $r'\,b$, and r and r' to the north side of it.

(9.) By aid of the micrometer screw bring the luminous image to its middle position on the scale.

(10.) Cause R B, B′ R′ to have different temperatures. The luminous image is seen to move in such a direction as is due to r approaching the cooler, and receding from the warmer of the two deflectors B R, B′ R.′

5. On a Constant Pressure Gas Thermometer. By Sir William Thomson, F.R.S.

In the article on "Heat" published in the eleventh volume of the *Encyclopædia Britannica*, referred to in my previous communications to the Royal Society on Steam Pressure Thermometers, it is shown that the Constant Pressure Air Thermometer is the proper form of expansional thermometer to give temperature on the absolute thermodynamic scale, with no other data as to physical properties of the fluid than the thermal effect which it experiences in being forced through a porous plug, as in the experiment of Joule and myself on this subject;* and the thermal capacity of the fluid under constant pressure. These data for air, hydrogen, and nitrogen have all been obtained with considerable accuracy, and therefore it becomes an important object towards promoting accurate thermometry, to make a practical working thermometer directly adapted to show temperature on the absolute thermodynamic scale through the whole range of temperature, from the lowest attainable by any

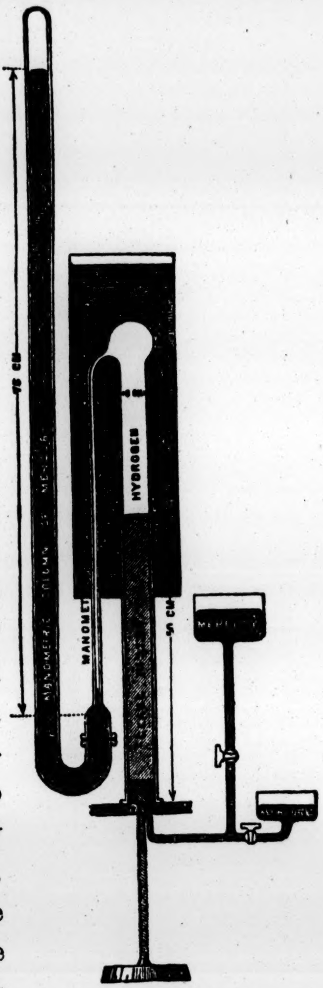
* "Thermal Effects of Fluids in Motion," Trans. Roy. Soc. Lond., June 1853, June 1854, June 1860, and June 1862.

means, to the highest for which glass remains solid. This, I believe, may be done by avoiding the objectionable expedient adopted by Pouillet and Regnault, of allowing a portion (when high temperatures are to be measured the greater portion) of the whole gas to be pressed into a cool volumetric chamber, out of the thermometric chamber proper, by the expansion of the portion which remains in; and instead fulfilling the condition, stated, but pronounced practically impossible, by Regnault ("Expériences," vol. i. pp. 168, 169), that the thermometric gas "shall like the mercury of a mercury thermometer be allowed to expand freely at constant pressure in a calibrated reservoir maintained throughout at one temperature." I have accordingly designed a constant pressure gas thermometer to fulfil this condition. It is represented in the accompanying drawing, and described in the following extract from the article referred to:—

The vessel containing the thermometric fluid, which in this case is to be either hydrogen or nitrogen,* consists in the main of a glass bulb and tube placed vertically with bulb up and mouth down; but there is to be a secondary tube of much finer bore opening into the bulb or into the main tube near its top, as may be found most convenient in any particular case. The main tube which, to distinguish it from the secondary tube, will be called the volumetric tube, is to be of large bore, not less than 2 or 3 centimetres, and is to be ground internally to a truly cylindric form. To allow this to be done it must be made of thick, well-annealed glass like that of the French glass-barrelled air-pumps. The secondary tube, which

* Common air is inadmissible, because even at ordinary temperatures its oxygen attacks mercury. The film of oxide thus formed would be very inconvenient at the surface of the mercury caulking, round the base of the piston, and on the inner surface of the glass tube to which it would adhere. Besides, sooner or later the whole quantity of oxygen in the air must be diminished to a sensible degree by the loss of the part of it which combines with the mercury. So far as we know, Regnault did not complain of this evil in his use of common air in his normal air thermometer nor in his experiments on the expansion of air ("Expériences," vol. i.), though probably it has vitiated his results to some sensible degree. But he found it to produce such great irregularities when, instead of common air, he experimented on pure oxygen, that from the results he could draw no conclusion as to the expansion of this gas ("Expériences," vol. i. p. 77). Another reason for the avoidance of air or other gas containing free oxygen is to save the oil or other liquid which is interposed between it and the mercury of the manometer from being thickened or otherwise altered by oxidation.

will be called the manometric capillary, is to be of round bore, not very fine, say from half a millimetre to a millimetre diameter. Its lower end is to be connected with a mercury manometer to show if the pressure of the thermometric air is either greater or less than the definite pressure to which it is to be brought every time a thermometric measurement is made by the instrument. The change of volume required to do this for every change of temperature is made and measured by means of a micrometer screw* lifting or lowering a long solid glass piston, fitting easily in the glass tube, and caulked air-tight by mercury between its lower end and an iron sole-plate by which the mouth of the volumetric tube is closed. To perform this mercury caulking, when the piston is raised and lowered, mercury is allowed to flow in and out through a hole in the iron sole-plate by an iron pipe, connected with two mercury cisterns at two different levels by branches each provided with a stopcock. When the piston is being raised the stopcock of the branch leading to the lower cistern is closed, and the other is opened enough to allow the mercury to flow up after the piston and



* This screw is to be so well fitted in the iron sole-plate as to be sufficiently mercury-tight without the aid of any soft material, under such moderate pressure as the greatest it will experience when the pressure chosen for the thermometric gas is not more than a few centimetres above the external atmospheric pressure. When the same plan of apparatus is used for investigation of the expansion of gasses under high pressures, a greased leather washer may be used on the upper side of the screw-hole in the sole-plate, to prevent mercury from escaping round the screw. It is to be remarked that in no case will a little oozing out of the mercury round the screw while it is being turned introduce any error at all into the thermometric result; because the correctness of the measurement of the volume of the gas depends simply on the mercury being brought up into contact with the bottom of the piston, and not more than just perceptibly up between the piston and volumetric tube surrounding it.

press gently on its lower side, without entering more than infinitesimally into the space between it and the surrounding glass tube (the condition of the upper bounding surface of the mercury in this respect being easily seen by the observer looking at it through the glass tube). When the piston is being lowered, the stopcock in the branch leading from the upper cistern is closed, and the one in the branch leading to the lower cistern is opened enough to let the mercury go down before the piston, instead of being forced to any sensible distance into the space between it and the surrounding tube, but not enough to allow it to part company with the lower surface of the piston. The manometer is simply a mercury barometer of the form commonly called a siphon barometer, with its lower end not open to the air but connected to the lower end of the manometric capillary. This connection is made below the level of the mercury in the following manner. The lower end of the capillary widens into a small glass bell or stout tube of glass of about 2 centimetres bore and 2 centimetres depth, with its lip ground flat like the receiver of an air-pump. The lip or upper edge of the open cistern of the barometer (that is to say, the cistern which would be open to the atmosphere were it used as an ordinary barometer) is also ground flat, and the two lips are pressed together with a greased leather washer between them to obviate risk of breaking the glass, and to facilitate the making of the joint mercury tight. To keep this joint perennially good, and to make quite sure that no air shall ever leak in, in case of the interior pressure being at any time less than the external barometric pressure or being arranged to be so always, it is preserved and caulked by an external mercury jacket not shown in the drawing. The mercury in the thus constituted lower reservoir of the manometer is above the level of the leather joint, and the space in the upper part of the reservoir over the surface of the mercury, up to a little distance into the capillary above, is occupied by a fixed oil or some other practically vapourless liquid. This oil or other liquid is introduced for the purpose of guarding against error in the reckoning of the whole bulk of the thermometric gas, on account of slight irregular changes in the capillary depression of the border of the mercury surface in the reservoir.

In the most accurate use of the instrument, the glass and mercury and oil of the manometer are all kept at one definite temperature, according to some convenient and perfectly trustworthy intrinsic thermoscope, by means of thermal appliances not represented in the drawing but easily imagined. This condition being fulfilled, the one desired pressure of the thermometric gas is attained with exceedingly minute accuracy by working the micrometer screw up or down until the oil is brought precisely to a mark upon the manometric capillary.

In fact, if the glass and mercury and oil are all kept rigorously at one constant temperature, the only access for error is through irregular variations in the capillary depressions in the borders of the mercury surfaces. With so large a diameter as the 2 centimetres chosen in the figured dimensions of the drawing, the error from this cause can hardly amount to $\frac{1}{100}$ per cent. of the whole pressure, supposing this to be one atmo or thereabouts.

For ordinary uses of this constant-pressure gas thermometer, where the most minute accuracy is not needed, the rule will still be to bring the oil to a fixed mark on the manometric capillary; and no precaution in respect to temperature will be necessary except to secure that it is approximately uniform throughout the mercury and containing glass, from lower to higher level of the mercury. The quantity of oil is so small that, whatever its temperature may be, the bringing of its free surface to a fixed mark on the capillary secures that the mercury surface below the oil in the lower reservoir is very nearly at one constant point relatively to the glass, much more nearly so than it could be made by direct observation of the mercury surface, at all events without optical magnifying power. Now if the mercury surface be at a constant point of the glass, it is easily proved that the difference of pressures between the two mercury surfaces will be constant, notwithstanding considerable variations of the common temperature of the mercury and glass, provided a certain easy condition is fulfilled, through which the effect of the expansion of the glass is compensated by the expansion of the mercury. This condition is, that the whole volume of the mercury shall bear to the volume in the cylindric vertical tube from the upper surface to the level of the lower surface the ratio of $(\lambda - \frac{1}{3}\sigma)$ to $(\lambda - \sigma)$, where λ denotes the cubic expansion of the mercury and σ the cubic expansion

of the solid for the same elevation of temperature, it being supposed for simplicity of statement that the tube is truly cylindric from the upper surface to the level of the lower surface, and that the sectional area of the tube is the same at the two mercury surfaces. The cubic expansion of mercury is approximately seven times the cubic expansion of glass. Hence

$$(\lambda - \tfrac{1}{3}\sigma)/(\lambda - \sigma) = (7 - \tfrac{1}{3})/6 = 1{\cdot}111\,.$$

Hence the whole volume of the mercury is to be about 1·111 times the volume from its upper surface to the level of the lower surface; that is to say, the volume from the lower surface in the bend to the same level in the vertical branch is to be $\frac{1}{9}$ of the volume in the vertical tube above this surface. A special experiment on each tube is easily made to find the quantity of mercury that must be put in to cause the pressure to be absolutely constant when the surface in the lower reservoir is kept at a fixed point relatively to the glass, and when the temperature is varied through such moderate differences of temperature as are to be found in the use of the instrument at different times and seasons.

A sheet-iron can containing water or oil or fusible metal, with external thermal appliances of gas or charcoal furnace, or low-pressure or high-pressure steam heater, and with proper internal stirrer or stirrers, is fitted round the bulb and manometric tube to produce uniformly throughout the mass of the thermometric gas the temperature to be measured. This part of the apparatus, which will be called for brevity the heater, must not extend so far down the manometric tube that when raised to its highest temperature it can warm the caulking mercury to as high a temperature as 40° C., because at somewhat higher temperatures than this the pressure of vapour of mercury begins to be perceptible, and would vitiate the thermometric use of the pure hydrogen or nitrogen of our thermometer. To secure sufficient coolness of the mercury it will probably be advisable to have an open glass jacket of cold water (not shown in the drawing) round the volumetric tube, 2 or 3 centimetres below the bottom of the heater, and reaching to about half a centimetre above the highest position of the bottom of the piston.

It seems probable that the constant-pressure hydrogen or nitrogen

gas thermometer which we have now described may give even more accurate thermometry than Regnault's constant-volume air thermometers, and it seems certain that it will be much more easily used in practice.

We have only to remark here further that, if Boyle's law were rigorously fulfilled, thermometry by the two methods would be identical, provided the scale in each case is graduated or calculated so as to make the numerical reckoning of the temperature agree at two points,—for example, 0° C. and 100° C. The very close agreement which Regnault found among his different gas thermometers and his air thermometers with air of different densities, and the close approach to rigorous fulfilment of Boyle's law which he and other experimenters have ascertained to be presented by air and other gases used in his thermometers, through the ranges of density, pressure, and temperature at which they were used in these thermometers, renders it certain that in reality the difference between Regnault's normal air thermometry and thermometry by our hydrogen gas constant-pressure thermometer must be exceedingly small. It is therefore satisfactory to know that for all practical purposes absolute temperature is to be obtained with very great accuracy from Regnault's thermometric system by simply adding 273 to his numbers for temperature on the centigrade scale. It is probable that at the temperatures of 270° or 300° C. (or 532 or 573 absolute) the greatest deviation of temperature thus reckoned, from correct absolute temperature, is not more than half a degree.

Monday, 3d May 1880.

PROFESSOR DOUGLAS MACLAGAN, Vice-President, in the Chair.

KEITH PRIZE.

The Chairman announced that the Council had awarded the Keith Prize, for the biennial period 1877–79, to Professor H. C. Fleeming Jenkin, for his Paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery," published in the Society's Transactions; Part II. having appeared in the volume for 1877–78.

The following Communications were read :—

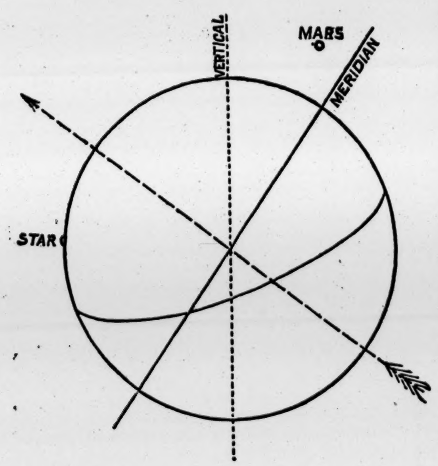
1. On the Occultation of the Star 103 Tauri. (B. A. C. 1572.) By Edward Sang.

An occultation of a star, though not appealing to ordinary observation with the same force, is intrinsically an event as striking as an eclipse of the sun. It establishes the fact of the moon's proximity. Were it not that the moon's brightness overpowers the light of the small stars, occultations would be commonplace phenomena. As things are, we can watch, with the eye unaided, the eclipses of the planets and larger stars, not down, perhaps, to below the third magnitude; and the rarity of such conspicuous objects makes the occultations correspondingly rare.

By help of a telescope of two or three feet in focal length, we are able to examine stars even so small as of the sixth magnitude, and thus greatly to increase the number of observations, so much so that as many as 150 occultations may be visible from one place in the course of the year.

The particular case to which I would draw attention is thus one of many: it derives its interest from the proximity of the planet Mars, whose occultations will have been carefully observed from many places.

The three objects—the moon, Mars, and the star—were all within the field of the telescope; their relative positions at the instant of the star's disappearance being as shown in the accompanying figure. The observation was made with a telescope having an aperture of 1·9 in., a focal length of 23·5 in., and a magnifying power of 26. The moon's dark edge was distinctly visible, the atmospheric tremor was slight, so that, notwithstanding the moon's proximity to the horizon, the disappearance was watched under very favourable circumstances. The time was noted by an excellent chronometer, which was compared, twelve hours thereafter,



with the mean time clock of the Royal Observatory, and again rated after nine days. The comparison of the time may be held as true to within a quarter of a second. The observation may be recorded thus:—

North Latitude, . . .	55° 55′ 42″
West Longitude, . . .	0h. 12m. 42s.

Greenwich mean solar time of disappearance—

1880, March, 17d. 12h. 23m. 55·8s.,

as by the mean time clock of the Royal Observatory of Edinburgh.

In order to compare this with the predicted motions, the moon's right ascension and declination were interpolated strictly from the hourly table in the "Nautical Almanac," and the star's place was taken from the "Elements of Occultations," while the parallax was computed on the supposition that the earth's equatorial is to the polar axis as 300 to 299. In this way the expected time was computed to be 17d. 12h. 23m. 46·3s., or 9·5s. earlier than the observed time.

There is no astronomical phenomenon more definite as to time than the occultation of a star, nor any perhaps more easily observed when the disappearance is against the dark edge of the moon. Provided that the telescope be sufficiently powerful to show the star, it is of little or no moment whether the definition be good, or even whether the instrument have been well adjusted to focus. In all cases the disappearance is instantaneous.

But, although the observation be thus satisfactory, there are various difficulties in the way of the calculations. In the first place, there is the error to which our lunar tables are liable; these tables, all founded on previous observation, are brought forward by an estimate of the laws and rate of change, and thus are unavoidably subject to a gradually increasing uncertainty. Wonderfully exact as these tables are, it would always be necessary, before drawing any exceedingly minute conclusion, to study the tabular error as obtainable from nearly contemporaneous observations on the moon. Next we have the possible error in the tabulated place of the star; an error, in the case of small stars, which is not to be

despised. The number of such stars is great, the number of observers small.

Next in order comes the size and shape of the earth. A relatively small difference in our position on the earth's surface makes a notable difference on the apparent position of the moon, and, in consequence, on the time of the occultation; even the height of the observer above the level of the sea has its influence. This may be well studied in the present instance; viewed from our latitude, the moon was seen to pass a little to the south of the planet Mars, whereas in the southern counties of England an occultation was seen. The oblateness of the earth has also to be taken into account. In the present instance calculations made as if the earth were spherical, would give the disappearance some eighteen seconds earlier than the above. Hence observations made on these phenomena from places in different latitudes afford a means for determining the earth's oblateness.

But, lastly, these observations are all deranged by the extreme jaggedness of the moon's edge. This jaggedness is well seen during an eclipse of the sun; it is also conspicuous against the disc of a planet. I recollect of witnessing an occultation of Saturn, some half a century ago, during which the corner of a lunar mountain was projected against the planet in such a way as to cut out a sector of about one-third of the surface. Irregularities of such magnitude cause serious variations in the times of disappearance and reappearance; and, for the purpose of estimating their possible extent, it might be useful to make concerted observations at places a few miles apart, so that the appulse may happen, here on the top of a lunar mountain, there in the hollow.

2. On Currents produced by Friction between Conducting Substances, and on a new form of Telephone Receiver. By James Blyth, M.A.

In former papers laid before this Society, I showed that when any two metals are rubbed against each other, a current of electricity is produced; and that this current agrees in direction with the thermo-electric current for the same two metals, and is greater, approximately at least, in proportion as the metals rubbed are far

apart on the thermo-electric scale,—the greatest current, as far as I have yet observed, being got from antimony and bismuth.

It is very difficult to decide as to the cause or causes of such currents. They may be (1) purely thermo-electric; (2) the currents, which are the supposed cause of friction; (3) currents produced by contact force between adhering films of air, moisture, or other substances with which the surfaces rubbed are tarnished; or (4) they may arise from all these causes combined. The following experiments were made in hopes of getting some information on these points.

My first experiment was to obtain the exact difference, as far as the production of a momentary current is concerned, between rubbing two pieces of metal together, and knocking the one against the other. For this purpose I repeated, with greater care, an experiment which I formerly described. It consisted in attaching a wire firmly to an ordinary hammer, and leading it to one of the terminals of a telephone circuit, while the wire from the other terminal was rigidly attached to a stiff bar of copper held vertically in a small table vice. When the face of the hammer was rubbed against the end of the copper bar, a very distinct grating noise was always heard in the receiving telephone; but the sound was almost inaudible when the bar was knocked by the hammer, if proper care were taken not to combine rubbing with knocking. This is, however, so difficult practically, that it is just possible that the sounds which I heard are due to faint rubs accompanying the knocking.

Should this not be the case, however, this difference of effect seems to show that the currents are not wholly, although they may be mainly, thermo-electric, as it is hard to believe that the heat produced at the junction of the surfaces by a smart blow can be less than that produced by a faint rub. Granting that the knocking is actually heard, it seems not unlikely that this effect may be due to the currents associated with rapid changes of form in matter. As has been remarked by Professor Tait (Proc. Roy. Soc. Edin. vol. ix. p. 552), these currents are such as would be capable of detection by the telephone.

In order to detect what effect, if any, the presence of the air had upon these friction currents, I employed the apparatus commonly called the *electric egg*. Having unscrewed the interior balls, I

fastened in their places two metallic strips, one of copper and the other of iron, so arranged that they could be made to rub against each other by moving the upper rod up and down in its air-tight socket. Before being fixed on, the metal surfaces were both well cleaned by scraping. When this apparatus was included in the circuit either of a galvanometer or telephone, no difference could be detected either in the deflection or the sound produced, by exhausting the air, as far as could be done, with an ordinary good air-pump. It is possible, however, that there may be films of air adhering to the metals which cannot be removed by pumping. Indeed, in the whole of this inquiry, the great difficulty is to be sure of what are the surfaces that are in contact.

Having ascertained that the current produced by the friction of antimony and bismuth is of some strength, and fairly constant when the friction is constant, I proceeded to make a small dynamo machine for producing currents on this principle. It consists of a cylinder of antimony 3 inches long and $2\frac{1}{2}$ inches in diameter, mounted on an axis which runs in centres. By a fly-wheel and band this cylinder is driven rapidly round against a plate of bismuth pressed tight against it by a stiff spring. Wires are led from the plate of bismuth and from one of the pivots on which the cylinder revolves to two binding screws, which form the electrodes of the machine. When this machine is included in the circuit with a microphone transmitter and a telephone, the current from it can be used for the transmission of musical sounds and even loud speaking. There is, however, heard along with the transmitted sound the noise arising from the friction of the antimony and bismuth. I have succeeded in transmitting, in this way, very distinctly, tunes played on a violin to which a microphone was attached. It is very curious, in this experiment, to hear so distinctly the music, notwithstanding the friction noise which accompanies it. It is to be noticed that the sound heard in the telephone of the rubbing of two pieces of metal together in a distant room is an effect precisely identical to this. In this case the rubbing produces the current, and the more or less loose contact of the metals acts as the microphone whereby the sound is transmitted through means of that current to the telephone.

I have also tried, with varying success, several other forms of this *friction current-producer*. In one of the most effective of these the

rubbing substances are arranged like a pair of mill-stones, the lower stone being a disc of iron laid horizontally, and the upper a disc of copper mounted on a vertical axis on which it can revolve. The surfaces are kept pressing against each other by a strong spring. When the upper disc is made to revolve rapidly, a very decided current is produced; and this I found to be markedly increased, as indicated by the telephone, by feeding in between the discs powdered antimony and bismuth combined. Of course we have here a series of rapid reversals of the current, as the direction of the current will depend upon whether particles of antimony or particles of bismuth are in contact with the lower plate. This clearly indicates a thermo-electric effect; and I have no doubt that the effect will be increased by applying a means whereby the upper surface of the copper plate and the lower surface of the iron one can be kept cold by a freezing mixture. As yet, however, I have not had time to try that. In another form I took two cylinders, the one of antimony and the other of bismuth, and placed them together end-wise, the pressure between them being regulated by a screw. The antimony cylinder was kept stationary, and the bismuth made to revolve very rapidly against it, so much so that both cylinders rapidly became hot. This also gave a pretty strong current.

Seeing that the friction between metals does certainly produce an electric current, it seemed natural to inquire whether an electric current sent from a battery across the surface between two metals would not modify the friction of the one against the other. I have tried to test this in a variety of ways, and the results leave me in doubt whether to attribute the indications which I have received to actual changes in the friction or to incipient fusion of portions of the surfaces together by the heat produced by the current, or to an effect similar to the Trevelyan rocker. In one experiment I made an inclined plane which carried a pair of parallel rails of copper about three quarters of an inch apart. The rails were hinged at the lower end, so that the plane could be set at any angle with the horizon. It was so arranged that the current from the battery could be sent up the one rail, through any conductor laid across the two, and down the other rail. The surfaces of the rails were made quite smooth. When a heavy piece of metal was laid across the rails, the angle of repose was the same both when there was and was not a

current passing. It was different, however, when a light body, such as a sewing needle, was put on. Then, when the current from three Bunsen cells was passing, the plane could be elevated considerably past the angle of repose for no current, before the needle rolled down. On examination I found that the needle was actually sticking to the copper; but that, in almost all cases, this sticking gave way without the angle being altered after the current had been taken off for some time, and the needle and copper allowed to come back to their normal temperature. In another experiment I employed a Bell telephone to enable me to detect any variation of friction when a current was passing between the rubbing surfaces. To the centre of the telephone disc was attached a long narrow strip of light wood; the object of making the strip so long being to remove the telephone as far as possible from the inductive action of the battery current which was to be used. To the other end of the strip was attached a flat piece of bismuth. This rested on the convex surface of a cylinder of antimony, which could be rapidly rotated. The battery current was sent through the antimony and bismuth by entering the antimony by the axis on which it revolved, and leaving the bismuth by a spring pressing tightly against it. In the battery circuit was included the violin with its microphone already mentioned, and the telephone with the rod attached was placed as the transmitting telephone in a telephone circuit. When the antimony cylinder was rapidly rotated, a listener in the receiving telephone watched attentively till his ear became accustomed to the sound produced by the rubbing, and transmitted along the wooden rod to the telephone disc. The battery circuit was then joined, and the violin played, the antimony cylinder meanwhile rotating at the same rate as before. No alteration in the sound was audible, which indicated no alteration in the friction. I then substituted a sharp point for the flat piece of bismuth, and immediately the violin sounds were faintly but clearly heard. This led me to think that some sticking was produced by the fusing of the sharp point, and more especially as the sound became a little clearer as the rotation became very slow.

Acting on this hint, it immediately occurred to me that a receiving telephone could be constructed depending upon this effect. I therefore took my bismuth cylinder and mounted it on a frame so that it could be made to rotate very truly on pivots. By wheels

and bands it was also made to rotate slowly. A phonograph mouthpiece, with a very thin disc of wood or mica, was next placed, so that a fine wire with a sharp point bent at a right angle, and with its other end attached to the centre of the disc just pressed with its sharp point on the convex surface of the bismuth cylinder. A current of four Bunsen cells was now passed through the wire and cylinder, and also through the violin microphone. When the violin was played the tune was heard faintly proceeding from the mouthpiece even when the bismuth cylinder was stationary. This arose simply from the loose contact of the wire and bismuth. The sound was, however, very greatly increased when the cylinder was rotated slowly,—so loud indeed, that it could be distinctly heard all over an ordinary room. I have been able to transmit singing very clearly, but not speaking clearly enough to be understood. This instrument is analogous to the loud-speaking telephone of Mr Edison; but the explanation of their action must be very different if electrolysis, as is usually supposed, be the cause of the variation in the slipping of the platinum point on the chalk cylinder, which is characteristic of Edison's instrument. Quite recently the electrolytic action has been questioned, and a different explanation given by Professor Barret of Dublin. It is evident that electrolysis can in no sense come into play when the cylinder and rubbing point are both metallic. In that case two probable explanations of the action readily suggest themselves. The one is that there is more or less of an actual sticking of the metals together, arising from their fusion by the heat of the current. If this be so, then, the loose contact is alternately made a very good one, and then one actually broken. The other is the action of the Trevelyan rocker. Here, however, we have clearly only an analogous, and not by any means an identical effect. In the Trevelyan rocker the heat passes from a large mass of hot metal through two points of contact to a cold block, whereas, in the other case the heat is only produced at the surfaces of separation, the temperature of the rest of the metals being almost unaffected. Still it appears to me that the variations of the heat at this point has a great deal to do with the actions of all microphones, and in general with all sounds transmitted from one loose contact to another. This is shown by substituting cylinders of different metals for the bismuth cylinder above mentioned, all other things remain-

ing the same. I have tried in this way, besides bismuth, cylinders of lead, tin, iron, antimony, and carbon, and find that bismuth gives by far the best result. In the other cases the sound from the simple loose contact is heard clearly enough; but there is hardly any increase of it produced by rotating the cylinder. Now this seems to be due in great part, if not entirely, to the difference between the metals as regards their specific heat and thermal conductivity. Obviously, with the same current, the greatest heat will be produced at the junction of the rubbing point and cylinder, when the specific heat and thermal conductivity are both as low as possible. Hence very probably the reason why bismuth answers so well, seeing that of all common metals it stands lowest on the list both in specific heat and thermal conductivity. In fact, if we take the product of the reciprocals of the specific heats and thermal conductivities of the above-mentioned metals, we find the product for bismuth greatly in excess of that for any of the others.

3. Note on the Present Outbreak of Solar Spots. By Professor Piazzi Smyth.

4th April 1880.

The physical activity going on in the Sun is still increasing, and worthy of all admiration. There was a very large spot had come round on the North-following limb on March 29; and was after that the subject of observation from day to day as it approached the central Solar meridian. But when it arrived there, on April 3, behold two less large but still most notable spots had burst out clear and full within the previous twenty-four hours between the great spot and the preceding limb. And on this day, April 4, there are two more notable ones very close to the greatest spot, making in all five remarkable spots not only all visible at once, but working and seething positively before our eyes.

April 5, Noon. Three of yesterday's five spots are gone. Faculæ are in their place; and that is "the end of spot-life," says Prof. Alex. S. Herschel.

4. Positive and Negative Electric Discharge between a Point and a Plate and between a Ball and a Plate. By Alexander Macfarlane, M.A., D.Sc., F.R.S.E.

I have made the following observations in the Natural Philosophy classroom of the United College, St Andrews, with the view of ascertaining whether the electromotive force required to cause a spark to pass between a small globe and a plate, or between a point and a plate, differs for the two kinds of electricity. Sir William Thomson suggested that I should apply to this question the method of measuring the electromotive force required to produce sparks, which I have described in papers already contributed to the Society (Trans. Roy. Soc. Edin. vol. xxviii. p. 633). It is a problem to which Faraday attached great importance. He says at sect. 1523, vol. 1, of his *Experimental Researches in Electricity*: "The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarised condition which the molecules of the dielectrics concerned acquire." He records a great number of experiments on this subject in sections 1465–1525. He took sparks between a ball 0·25 inch in diameter and a ball 2 inches in diameter. When the large one was connected with a discharging train, the small one charged positively gave a much longer spark than when charged negatively; also the small ball charged negatively gave a brush more readily than when charged positively in relation to the effect produced by increasing the distance between the two balls (sect. 1489). When the interval was below 0·4 of an inch, so that the small ball gave sparks whether positive or negative, he could not, he says, observe any constant difference either in their ready occurrence or the number which passed in a given time. But when the interval was such that the small ball when negative gave a brush, then the discharges from it as separate negative brushes were far more numerous than the corresponding discharges from it when rendered positive, whether those positive discharges were as sparks or brushes (sect. 1490).* As he puts

* Drs De La Rue and Müller have found in the case of the discharge of their great chloride of silver battery that the discharge between a point

it further on, he found difficulty in determining "the relative degrees of charge which the small ball acquires before discharge occurs, that is, whether it acquires a higher condition in the negative, or in the positive state, immediately preceding that discharge." The method of our experiments, it appears to me, has solved this difficulty.

I employed a Thomson's divided ring reflecting electrometer, which has either half-ring insulated. In this instrument Professor Swan has substituted a glass dish with a tubulure for holding the sulphuric acid in place of the original leaden pan, in which the charge of acid was carried on pieces of pumice stone. The tubulure allows the acid to be filled in or taken out with convenience. A piece of platinum foil suspended in the acid, from the aluminium needle, completely checks the oscillations, and renders the instrument "dead beat." The terminals for connecting the divided rings, which originally projected downwards, now project upwards, and, to secure perfect contact, are in the form of a helical brass spring passing through a varnished glass tube. The instrument was set so that the needle when charged was symmetrically situated with respect to the two half-rings, and the scale was set so that the wire-image was in the middle. The Holtz machine by which the electricity was produced, was at a distance of 20 feet from the electrometer.

In the first series of observations the point used was the conical point of a rod of a Henley's discharger. It was connected by means of an insulated wire with one conductor of the Holtz machine. The plate used is of 7 inches diameter, and was attached to the other stem of the discharger in such a manner that the rod was always normal to it at its centre. Both the plate and the other conductor of the Holtz machine were in conducting connection with the earth. Either kind of electricity was obtained by charging the one or the other paper conductor of the machine by means of a small Leyden jar. As either half ring of the electrometer could be insulated and the other connected with the earth, readings were taken in both directions. This is the meaning of the entries in the record *connections direct* and

and a disc is much more continuous with the point negative than with the point positive (Phil. Trans. vol. clxix. p. 90).

connections reversed. The *deflection* in the table is the position of the wire-image immediately before the passing of the spark; and the *zero* is its position after the spark had passed, and the Leyden jars of the Holtz machine had been discharged. These jars were found to contain a residual charge, but its existence had only a very slight effect upon the position of the wire-image.

In the second series of observations, the arrangement differed only in this—that a spherical ball of ·5 inch diameter was attached to the end of the rod.

From the first series of observations, by taking the mean of the two opposite readings, we obtain the following results:—

Point and Plate.

Distance between Point and Plate.	Electromotive force for Positive Discharge (1).	Electromotive force for Negative discharge (2).	Ratio of (1) to (2).
½ inch	76·6	67·1	1·14
1 inch	86·3	76·2	1·13
2 inches	102·3	95·2	1·07

Thus the electromotive force for the positive discharge was always greater than for the negative, but the ratio approaches the more nearly to unity the greater the distance of the point from the large plate. Thus the difference in the electromotive forces appears to be due to the presence of the large uninsulated plate. The behaviour of the index showed that the discharge was not single, but consisted of a rapid succession of discharges, for it first attained a temporary maximum deflection and then a steady deflection slightly less than the maximum. The latter on account of its being capable of being observed with greater precision was the one recorded. The discharge was not silent, but accompanied with a slight hissing sound.

From the second table, by taking the mean of the two opposite readings, we obtain the following results:—

BALL AND PLATE.

Length of Spark.	Electromotive force for Positive Spark (1).	Electromotive force for Negative Spark (2).	Ratio of (1) to (2).
¼ inch	118·8	129·7	·92
½ inch	179·6	201·7	·89
¾ inch	219·2	227·3	·96
1 inch	234·6	234·3	1·00

Thus under the above conditions and for a range of spark up to ·5 inch, at least, the electromotive force required to produce the discharge is less when the ball is charged positively than when charged negatively. Within that range the discharge took place in the form of a single loud white spark, the index gave only one reading, and fell back after the passage of the spark almost to its ultimate position. But when the distance between the extremity of the ball and the plate was increased to ·75 inch, the charge being negative, hissing sparks, giving only very small discharges as indicated by the behaviour of the index, preceded the loud spark which gave complete discharge. When the charge was changed to positive, the distance remaining the same, no hissing discharges were observed preceding the loud discharge. This is an instance of the phenomenon to which Faraday refers, viz., that when the charged ball (under the above conditions) is positive, a longer spark can be obtained than when the charged ball is negative. When the distance was increased to 1 inch, hissing discharges preceded the loud discharge in both cases, but they were much more numerous in the case of the negative than of the positive charge.

The results of the observations appear to explain this phenomenon. They show that a charge of positive electricity requires a less electromotive force than a charge of negative, in order to force its way in the form of a spark (which is a complete discharge). A charge of positive electricity will therefore be able to discharge all together at a greater distance, provided we assume that the brushy spark begins at the same electromotive force for each.

The truth or falsity of this assumption appears to be capable of being established with ease by means of the method of these experiments.

From the end of the second table it appears that the ratio tends to change from being less than unity to being greater than unity, when the hissing discharges begin to appear. Suppose the negative spark preceded by hissing discharges, but the positive not. Then the occurrence of these hissing discharges is apt to diminish the deflection at the time when the negative spark passes, while their absence in the case of the positive spark allows the full deflection to be observed. Thus the ratio of the readings may change to be greater than unity. In the case of the positive spark, the electromotive forces for the four distances lie well on the curve which from previous experiments we found to be true for the discharge between a ball and a plate, but in the case of the negative spark, only those for the first two distances.

The discharge from the point is a more complex phenomenon than the discharge from the ball; its explanation probably requires many further experiments.

These results accord with those published by Drs De La Rue and Müller, in Part I. of their "Research." They state (Phil. Trans. vol. clxix. p. 76) that with high tensions—5000 to 8000 chloride of silver cells—the spark between a point and a disc is longer when the point is positive, but with low tensions up to 3000 cells, it is generally longer when the point is negative. For the discharge they observed is single, like that which we obtained between the ball and plate, at the smaller distances, not intermittent like that which we obtained between the point and plate.

I may mention that the observations of April 10, 1880, are supported by a previous series of observations, taken by means of a more roughly divided scale.

The inductric and inducteous balls (to employ terms invented by Faraday), by which the measurement was effected, were at a distance from one another of 15 inches; and the length of the wire connecting the inducteous ball with the electrometer was about 10 feet.

In the record of observations the entries are given in the order in which they were observed.

RECORD OF OBSERVATIONS.

10*th April* 1880.

Table I.—Point and Plate.

Length of Discharge.	Charge on Point.	Connections of Electrometer.	Deflection.	Zero.	Difference.	Mean.	Ratio of Positive to Negative.
½ inch	positive	direct	279	355	76		
,,	,,	,,	282	360	78	77·7	
,,	,,	,,	290	369	79		
,,	,,	reversed	425	350	75		
,,	,,	,,	423	346	77	75·5	1·12
,,	,,	,,	421	347	74		
,,	negative	,,	265	335	70		
,,	,,	,,	264	332	68	69·3	1·16
,,	,,	,,	261	331	70		
,,	,,	direct	423	358	65		
,,	,,	,,	427	362	65	65·0	
,,	,,	,,	430	365	65		
1 inch	negative	direct	453	378	75		
,,	,,	,,	454	377	77	75·5	
,,	,,	,,	457	383	74		
,,	,,	reversed	265	342	77		
,,	,,	,,	265	342	77	77·0	1·16
,,	,,	,,	265	342	77		
,,	positive	,,	443	353	90		
,,	,,	,,	440	352	88	88·0	1·10
,,	,,	,,	438	352	86		
,,	,,	direct	263	344	81		
,,	,,	,,	260	349	89	84·7	
,,	,,	,,	265	349	84		
2 inches	,,	,,	247	351	104		
,,	,,	,,	253	355	102	102·0	
,,	,,	,,	255	355	100		
,,	,,	reversed	452	350	102		
,,	,,	,,	453	350	103	102·7	1·10
,,	,,	,,	448	345	103		
,,	negative	,,	224	322	98		
,,	,,	,,	227	317	90	92·7	1·05
,,	,,	,,	227	317	90		
,,	,,	direct	460	365	95		
,,	,,	,,	459	359	100	97·7	
,,	,,	,,	460	362	98		

TABLE II.—SMALL GLOBE AND PLATE.

Length of Spark.	Charge on Globe.	Connections of Electrometer.	Deflection.	Zero.	Difference.	Mean.	Ratio of Positive to Negative.
¼ inch	negative	direct	505	367	138		
,,	,,	,,	500	370	130	134·7	
,,	,,	,,	505	369	136		
,,	,,	reversed	215	342	127		
,,	,,	,,	219	339	120	124·7	·91
,,	,,	,,	218	345	127		
,,	positive	,,	473	347	126		
,,	,,	,,	475	350	125	122·3	·92
,,	,,	,,	470	354	116		
,,	,,	direct	235	345	110		
,,	,,	,,	230	345	115	115·3	
,,	,,	,,	228	349	121		
½ inch	positive	direct	170	340	170		
,,	,,	,,	165	340	175	173·3	
,,	,,	,,	170	343	173		
,,	,,	reversed	550	360	190		
,,	,,	,,	545	365	180	186·0	·90
,,	,,	,,	545	357	188		
,,	negative	,,	125	313	188		
,,	,,	,,	119	312	193	191·7	·88
,,	,,	,,	118	312	194		
,,	,,	direct	580	368	212		
,,	,,	,,	590	379	211	211·7	
,,	,,	,,	590	378	212		
¾ inch (1)	,,	,,	610	390	220		
,,	,,	,,	613	394	219	220·7	
,,	,,	,,	613	390	223		
,,	,,	reversed	100	335	235		
,,	,,	,,	100	333	233	234·0	1·01
,,	,,	,,	98	332	234		
,, (2)	positive	,,	580	370	210		
,,	,,	,,	600	370	230		
,,	,,	,,	580	365	215	222·5	·92
,,	,,	,,	600	365	235		
,,	,,	direct	115	337	222		
,,	,,	,,	125	338	213		
,,	,,	,,	120	333	213	216·0	
1 inch (3)	,,	,,	105	335	230		
,,	,,	,,	110	330	220	224·0	
,,	,,	,,	105	327	222		
,,	,,	reversed	605	353	252		
,,	,,	,,	610	365	245	245·3	·90
,,	,,	,,	605	366	239		
,, (4)	negative	,,	65	315	250		
,,	,,	,,	65	315	250	247·7	1·11
,,	,,	,,	70	313	243		
,,	,,	direct	610	380	230		
,,	,,	,,	610	392	218	221·0	
,,	,,	,,	610	395	215		

(1.) Brushy sparks giving an incomplete discharge and preceding the white spark first observed. (2.) No brushy sparks observed before the white spark. (3.) Brushy sparks observed before the white spark. (4.) Great number of brushy sparks before the white spark.

6. Researches in Thermometry.

By Edmund J. Mills, D.Sc., F.R.S. (Communicated by Professor Crum Brown.)

(*Abstract.*)

Having had occasion in the course of my work to investigate some of the properties of the mercurial thermometer, I have obtained a series of results which are comprised in a memoir now submitted to the Society. A brief summary of these is given in the following abstract.

1. After describing a simple method of calibrating a thermometer, I give a detailed proof (following Pierre) that the calibration unit gradually undergoes a slight diminution in value. In the course of five years this may amount, in a new thermometer, to as much as ·21 per cent. The 0°–100° interval, therefore, requires periodic verification.

2. When the indicating part of a thermometer has a different temperature from the bulb, an "exposure" correction becomes necessary. If y represent the value of this correction, it is generally determined from the equation—

$$y = \cdot 000\,1545(\mathrm{T} - t)\mathrm{N}\,;$$

where ·000 1545 is the difference between the co-efficients of cubical expansion of glass and mercury, T is the reading of the thermometer, and t is the mean temperature of the exposed portion N. Experimental evidence is adduced in the memoir to show that the factor of (T–t) N is not a constant quantity, but a linear function of N. The equation thus becomes—

$$y = (\alpha + \beta\mathrm{N})(\mathrm{T} - t)\mathrm{N}\,,$$

where α and β are constants to be determined from the experiments. The values of α and β are very small, and from about 1500–2000 eye-observations were required to determine them, according to the instrument employed.

3. The gradual ascent of the zero with time can be expressed by a logarithmic curve having two terms, viz.—

$$y = \mathrm{A}\alpha^{x} + \mathrm{B}\beta^{x}\,,$$

where y is the remaining ascent, x the time, and A + B the total ascent. After two or three intervals x, the value of $B\beta^x$ is usually inconsiderable; so that the ascent may at an early period be represented by the simple expression—

$$y = A\alpha^x .$$

The probable error of a single comparison of theory with experiment is, as a mean result of eleven curves, 0°·012 C. Incidentally, it is shown that the above law is obeyed whether the thermometer is vacuous or open to the air. In a similar manner, the movements of the zero with temperature can be expressed by the equation—

$$y = A\alpha^x - B\beta^x ,$$

where x represents equal successive intervals of temperature. In this case, the value of $B\beta^x$ is always appreciable. The probable error of a single comparison of theory with experiment is, as deduced from four curves, 0°·023 C.

Under the influence of heat, the zero of a vacuous thermometer at first descends, until the heat reaches a definite degree for each instrument; the "mean degree" observed was 154°. After this, the material of the bulb becomes, I think, semi-plastic and gives way to atmospheric pressure, the zero then rising. This phenomenon continues until the mercury has a sensibly strong vapour tension, which causes enlargement of the bulb and depression of the zero. A thermometer open to the air and kept upright, should, of course, on the application of an increasing heat, exhibit nothing but depression in the zero; and this is shown to be practically the case.

4. The correction known as Poggendorff's is then alluded to, and the importance of habitually employing it is distinctly pointed out.

5. The results of compressing a thermometer's bulb show that, up to 134 atmospheres, the effect is a linear function of the pressure. The instrument is in fact an excellent pressure-gauge.

6. In the course of the memoir, an apparatus is described for comparing the mercurial with the air thermometer, and the results of the comparison are stated. Attention is drawn to a compound

bath, having a principle believed to be new, and probably available in many cases where the maintenance of an exact temperature is required.

7. Reference is lastly made to an investigation of the melting-points of certain easily accessible and purifiable chemical substances, for which the results of the present researches have been utilised. The calculations are not quite complete; but I trust to place, at an early date, these important constants in the hands of physicists. The extremely tedious work of comparing the mercurial with the air thermometer will then, for a considerable length of scale, be in the future to a great extent avoided.

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Professor J. H. Scott, Otago; Mr James Graham; and Dr R. Sydney Marsden.

Monday, 17th May 1880.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Preliminary Notice of a Method for the Quantitative Determination of Urea in the Blood. By John Haycraft, M.B., B.Sc.

The subject of these investigations, some of the results of which are before the Society to-night, was suggested to me by Professor Carl Ludwig, and was carried out in his laboratory at Leipzig, where I worked with the help of his assistant, Dr Drechsel, to whose kindness and large knowledge of the subject I was much indebted.

In this paper I shall give simply an account of the method of analysis, leaving for the future a record of the facts which it has enabled me to obtain.

The estimation of a quantity of urea in a pure form, or in a

watery solution of fair strength is not difficult; nothing, indeed, can well be easier. There are a number of methods which we might employ, and which are exact and easy of application; such are those of Liebig, of Heintz, and Ragsky, of Bunsen, of Huefner, and of Davy, all of which may be applied for its estimation in urine.

With the blood it is another question, for here this substance exists in very small quantities (3 parts in 10,000), mixed with a mass of organic matter, from which it has first to be separated in a tolerably pure form before the quantity can be ascertained. This separation from the blood is the difficult task to be overcome, the estimation of the quantity will then be easy.

The subject is one of great importance in physiological chemistry, as all will admit, for it is a key to our knowledge of the metabolism of albuminous substances in the body, nearly all the nitrogen which is excreted passing out in this form.

This importance has been thoroughly appreciated by chemists, and many men of note have turned their attention to this subject.

It was Sir Robert Christison who first gave the stimulatus to modern inquiry, for he it was who found a large amount (much above the normal) of urea in the blood of patients suffering from Bright's disease.

His method did not pretend to great chemical accuracy, for he took only the clear serum of the blood, from which he crystallised out the urea as a nitrate after concentration.

This discovery ranks with the highest that chemists have made in their investigations of the healthy and diseased tissues, and few indeed have been the facts gleaned gradually, and with difficulty in later years.

Physiologists have endeavoured, but in vain, to found an accurate analytical process for its determination in blood, in order to investigate the many changes which occur in its amount during different physiological and pathological conditions. Lecanu, Marchand, Simon, Millon, Pettenkofer, Joseph Picard, Gscheidlen, and Drechsel have all worked with this object in view.

Indeed the chemistry of blood is beset with difficulties, as all

will admit; so many nitrogenous substances exist in it; these are closely allied one to another in their chemical relations, hence their separation is very difficult; and, lastly, during the process of separation, one substance may be changed into another.

In the case of urea estimation, fresh obstacles stand in our path which it is necessary to understand, in order that their removal may be attempted. Urea not only entirely decomposes when heated over 120° C., but when a watery solution is evaporated to dryness, part of it decomposes, producing a loss which varies of course with the quantity of water and the strength of the solution. A fraction of a gramme evaporated in a litre of water loses from 3 to 4 per cent. Now it is necessary to separate the albumen of blood from the urea, which entails the addition of much fluid, which fluid has to be evaporated down when decomposition of part of the urea ensues.

A common way is to coagulate the albumen with hot alcohol when three volumes of spirit are at least required. With acidulated boiling water, six or seven volumes are necessary for the complete coagulation. Besides this, the decomposition on evaporation is much increased if other organic impurities are present in the fluid. So much so, that if ordinary defibrinated undiluted blood be evaporated in flat dishes, even with a gentle heat, not a trace of urea is to be discovered in the hard black cake which results. Nay, a large quantity of pure urea may have been previously added, the whole decomposing during the evaporation. This is also the case with the watery and alcoholic extracts of urea from blood, for these contain much extractive matter of which the urea forms but a small portion. The loss which occurs during these evaporations is far more than would occur were the urea alone present in an alcoholic or watery solution.

Another difficulty in our way is that no substance was known which might be useful in its extraction, and in which it is insoluble. It is often thought to be insoluble in sulphuric ether, but this is far from the truth; indeed it is so soluble that ether can never be used to separate, say fat from it, in an analysis which professes to be quantitative. Urea, it may be stated, is very soluble in water and alcohol, and is soluble also in chloroform and acetic ether.

My first endeavour was to find a liquid in which urea is insoluble. A number were tried, and at last a substance, petroleum naphtha (a naphtha lately imported from America, and which is sold with other naphthas under the name of benzoline), was found in which urea is quite insoluble, and by means of which it is possible to separate urea from oil when both are in alcoholic solution. This petroleum naphtha, or benzoline, as we shall afterwards see, is of great service.

An endeavour was now made to prevent or lessen the loss which occurs on evaporating a solution of urea.

It was thought that possibly the high temperature used was prejudicial, and accordingly evaporations were conducted with solutions of known strength at a temperature lower than is ordinarily used. The solutions were evaporated on a water bath in flat dishes at a temperature of from 40° to 50° C., but, unfortunately, this was of little avail, the difference in the result being but very slight.

Failure also resulted when the urea solution (not an artificial one) was evaporated at a very low temperature and with diminished atmospheric pressure. The solution was acidified with acetic acid with loss following its evaporation.

Urea forms with acids salts of definite composition, and of well-marked crystalline form.

It was thought that by changing this substance into an oxalate or nitrate these might prove more stable, and as the ordinary methods admit of their estimation in these forms, the process might succeed, or at any rate it would be easy to reduce them as a last step into urea again.

A partial success was the result of this trial; for on evaporating an alcoholic solution of oxalate of urea (0·1 gramme per litre) there was not quite 2 per cent. loss. Indeed, a certain degree of decomposition seemed inevitable, and no way was found out of the difficulty. No method which involved evaporation could be said to be perfectly accurate. It is still possible to reduce this loss by using as little fluid as possible, and to obtain the urea unmixed with other organic solids, the presence of which is so prejudicial to the accuracy of our results.

To separate the albumen from blood, either with boiling alcohol

or hot acidulated water, requires a dilution of the blood with several volumes of water as we have seen; worse than this the alcoholic or watery extract of urea thus obtained contains so many extractives that the urea passes through several processes before it is in a fit condition to estimate, loss occurring during each process.

Can we lessen or altogether prevent this loss is the question before us?

Hearing that tungstic acid does not precipitate urea, and knowing also that it precipitates most organic substances, I tried to separate the urea by this agent.

Diluting with only $\frac{2}{3}$ vols. it was possible very completely to separate the albumens as a fine granular mass by the addition of tungstate of soda and acetic acid.

The after process, however, was so complicated (the urea had to be separated from the tungstic acid, and acetate of soda, and many extractives) that the loss was as great as before.

The separation by dialysis had long been thought of, for it was naturally suggested by the epithelium of the kidneys, by which urea is separated during life from the blood.

It had not been tried, however, as it promised to be tedious. Hearing, however, from Professor Browne that by placing blood into a dialyser with alcohol in the outer vessel it became quite hard and dry in a few hours, encouragement was given to a trial of this method.

Urea being very soluble both in alcohol and water, it would probably pass out with the fluid parts of the blood into the alcohol; a trial proved this to be the case, and a few experiments sufficed to found the method which will now be described.

80 c.c. or 100 c.c. of defibrinated blood are placed within a dialyser so as to form a thin layer on the parchment paper. The dialysers that I use are of glass, the inner vessel having a diameter of 8 to 9 inches, the outer one-half or three quarters of an inch more. The blood must not form a layer more than about 3 mm. thick, otherwise the lowest stratum will become dry and impervious while the upper will still remain fluid.

One hundred c.c. of alcohol are poured into the outer vessel, and the whole is covered. In from four to eight hours the alcohol

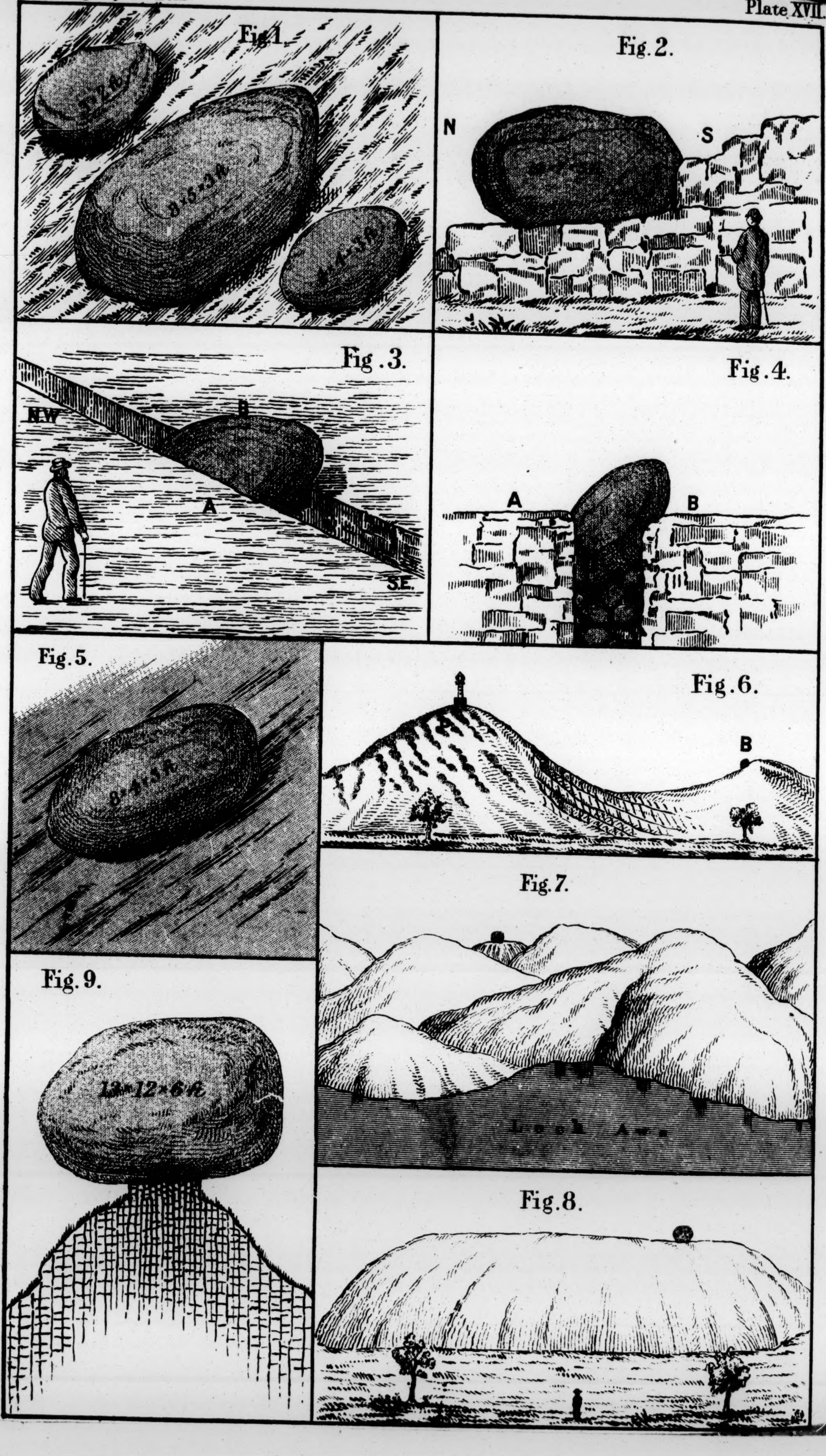
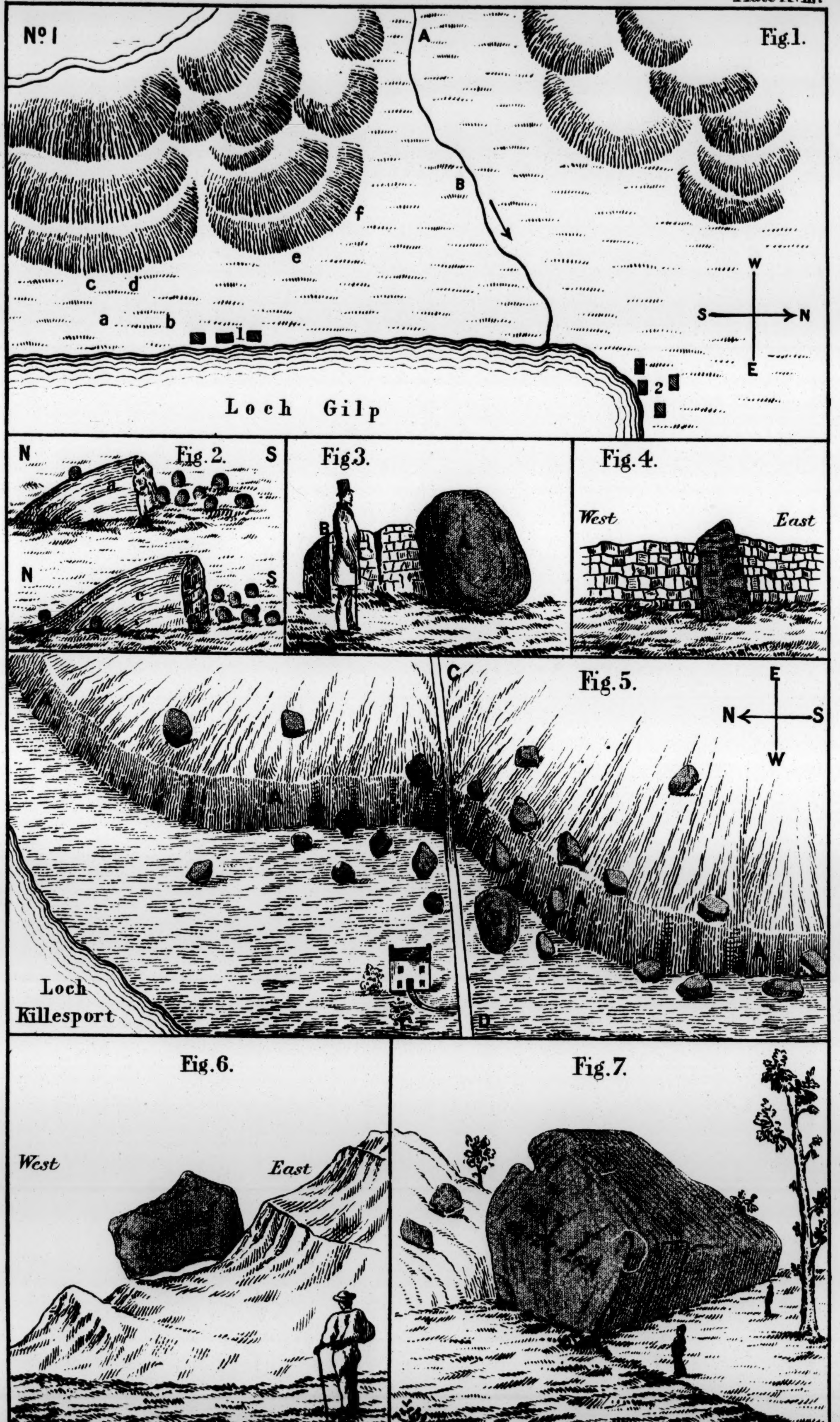


Fig. 1.
8×5×3 ft
Fig. 2.
N
S
Fig. 3.
N.W.
B
A
S.E.
Fig. 4.
A
B
Fig. 5.
8×4×3 ft
Fig. 6.
B
Fig. 7.
Loch Awe.
Fig. 8.
Fig. 9.
13×12×6 ft



W. & A. K. Johnston, Edinburgh & London.

Fig.1.

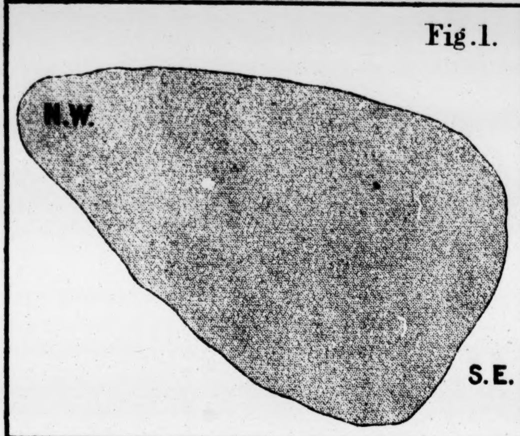


Fig.2.

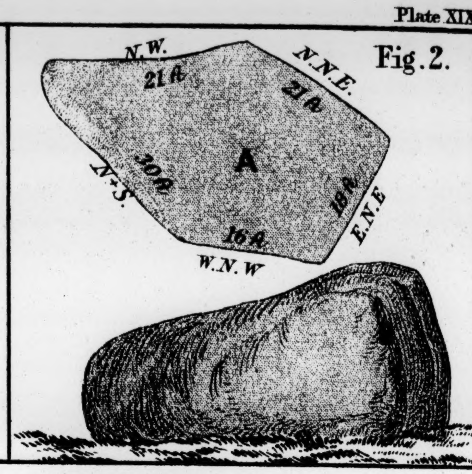


Fig.3.

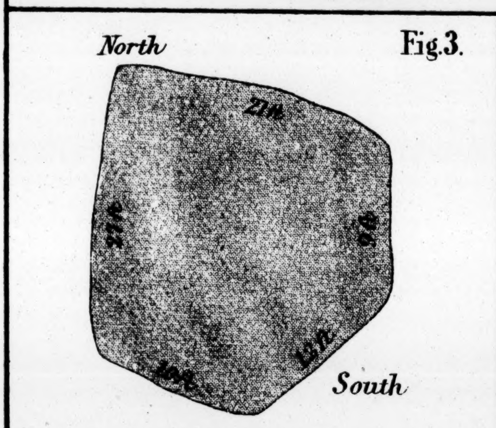


Fig.4

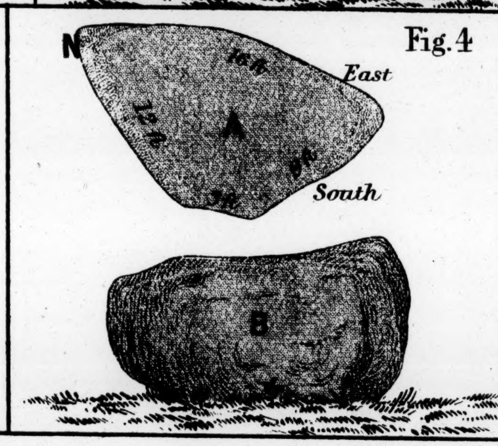


Fig.5.

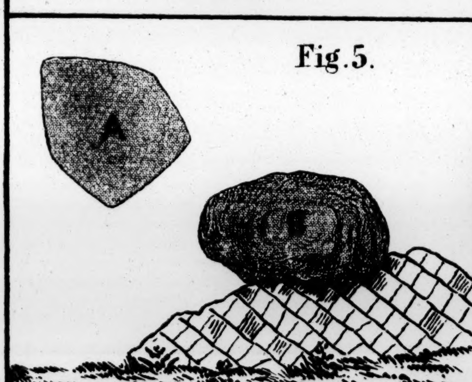


Fig.6.

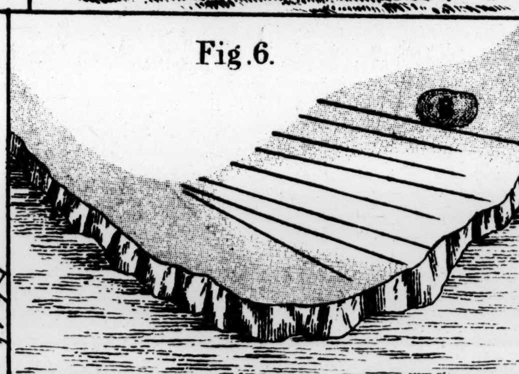


Fig.7.

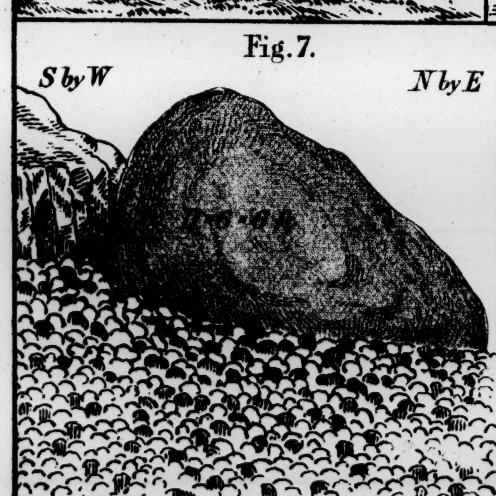
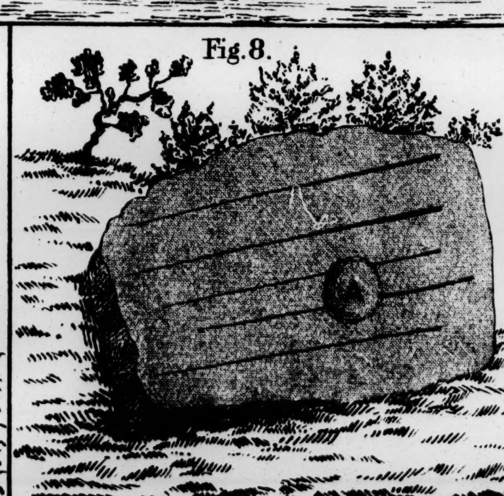


Fig.8.



in the outer vessel has risen, being increased in volume by the fluid parts of the blood, which now forms a dark tough cake within the dialyser, which, as a rule, sticks to the parchment paper. Of course this contains still much urea, and it must be washed. It is not sufficient to pour fresh water upon it; it must be detached from the parchment paper and bruised with fresh water in a mortar. For this purpose the parchment paper with the blood attached is brought into a flat dish gently heated over a water bath, when the blood may be detached with the help of a little warm water. Fresh parchment paper is now placed upon the ring of the dialyser, and the bruised mass brought into it and placed again in the alcohol. The blood soon becomes dry again from abstraction of water, and the alcohol is now evaporated after acidification with oxalic acid.

The blood in the dialyser is now warmed to drive off the little alcohol that is mixed with it, and is replaced in the dialyser with some more water, with which it can now be mixed with a curved spatula; for now it forms a finely granular mass, and does not cling to the parchment paper. When this is dry it is washed in the same way for the last time.

The alcohol here is also evaporated and the residue extracted with a little alcohol which is evaporated. An all-important point is this, that mixed with the urea, which may even now be seen on the dish, there is but little organic matter. Most of this can now be removed by washing the residue with petroleum naphtha. It is then extracted with acetic ether and the urea estimated.

I accomplished this in Germany by a method introduced by Bunsen; the urea is heated with an ammoniacal solution of chloride of barium in sealed tubes at 200° C.; decomposition of the urea into ammonia and carbonic acid occurs, and the amount of this is determined by weighing the carbonate of barium which is formed.

Lately I have used a shorter and more suitable method introduced by Huefner. The urea is mixed with a solution of hypobromide of soda, when carbonic acid and nitrogen are formed. The latter is collected, and the quantity calculated where the amount of urea may be inferred. There is loss with this process of about

7 per cent., for by calculation 1 gramme of urea should yield 300 c.c. of nitrogen at 0° C. and with 760 mm. of pressure.

It only yields, however, in practice, 340 c.c., the deficit is, however, very constant.

By adopting a modification of this, for which I am indebted to Professor A. Gamgee, almost all the nitrogen is given off. This is the addition of ordinary cane sugar syrup, when you obtain 363·4 c.c. of nitrogen.

This process was, of course, tested with great care in order to find out whether it was sufficiently accurate. Two equal portions of the same blood were taken, and to one was added a known quantity of pure dry urea. If the method was perfectly accurate the results of the two fluids when analysed should show a difference equal to the quantity of urea added.

That this was not the case is evident; for although the method approaches perfection, yet it is far from being perfect, and it would only lead to disappointment if its faults were passed lightly over.

Solutions of urea have still to be evaporated although in a comparatively pure form and with far less fluid. This, as we have shown, means loss.

This loss, however, is brought to a minimum, and is as a result of my test analyses not more than 7 per cent. or 8 per cent. Now, as part of this loss is constant, and as urea is actually obtained and estimated as such, the fallacy is less than these numbers would indicate.

That this method will ere long be supplanted by another and a better one I have no doubt, and, indeed, while working at its application to the investigation of physiological conditions, I am at the same time continually trying to improve the method. So exact, however, is it that one may always readily obtain a demonstration of urea from so small a quantity of blood as 10 c.c. I have here a specimen of urea in a very pure form from 15 c.c.: this has been frequently purified, and is mixed now with little else; one might almost estimate its quantity by weight. I may state that in blood far more urea is present than is ordinarily imagined. I have obtained 56 parts per 100,000, which is a very large quantity. The normal is between 40 and 30 part 100,000.

In summing up, this method is, I believe, reliable, but not sufficiently accurate to investigate minute changes.

I have already gleaned several important facts from its application, which I hope will form the subject of another paper. As a footnote I may add that the alcohol in the dialyser is green, and on evaporation green flakes are deposited; these are in many respects like bile pigment, but I have not worked sufficiently at the subject to give a dogmatic statement. It is stated that bile pigments do not exist in the blood. I shall not here enter upon the subject, as it would be premature; the point is simply mentioned, as, if my experiments are repeated, surprise might be expressed that I had not observed this fact.

A remark may be made here upon the separation of albumen by acidulated boiling water. I have reason to believe that this is a method which is thoroughly bad, although it is one which is constantly had recourse to in two or three branches of blood analysis. The coarseness of the coagulum varies with the temperature of the water.

If blood be poured into water which is thoroughly boiling the albumen collects in masses the size of a small bean, or sometimes in much larger pieces. If the water be not so hot, then the coagulation is finer.

Now, it is extremely difficult to wash out extractives from these albuminous masses, the difficulty increasing with their size. Hence your results may vary not with the amount of substance present in the blood, but with the coarseness of the coagulum, which is not desirable. I have tested this in the case of urea, and my friend Dr Bleile confirms the fact in the case also of sugar.

2. On the Phenomena of Variation and Cell-Multiplication in a species of Enteromorpha. By Patrick Geddes. Communicated by Professor A. Dickson.

3. On the Accurate Measurement of High Pressures. By Professor Tait.

In the course of an examination of some of the "Challenger" deep-sea thermometers, I have recently had occasion for measurements, accurate to one or two per cent., of pressures such as five or six tons weight per square inch. The ordinary gauges showed themselves to be quite untrustworthy, and it was necessary to devise some plan of whose accuracy the experimenter can feel assured. The following process has proved completely successful, and is capable of any desired degree of accuracy.

Simple methods based on the compression of gases, such as air or nitrogen, are of the highest value wherever they can be adopted; for the law of compression of these bodies is known with great accuracy (at least for one definite temperature) from the measurements recently made by Amagat, in which the pressures were directly reckoned in terms of a column of mercury. A simple form of gauge, in which the column of mercury compressing the gas into a small bulb at the extremity is made to break off at a constriction in the connecting tube, enabling us (by weighing the mercury forced over into the bulb) to measure the compression very accurately, suffices amply for all pressures up to a ton weight per square inch, or even farther.

But this instrument becomes rapidly less and less sensitive at higher pressures; so that though the law of compression for a considerably extended range is now known, for pressures above a ton something else is required.

Hooke's Law now comes to our assistance. An instrument resembling a thermometer in form (but with a tube of much larger section compared with the capacity of the bulb than is usual in mercury thermometers) supplies the next step. It is filled with mercury (because of the small expansibility of that liquid), and is thus practically unaffected by small changes of temperature. Over the mercury in the stem is a long column of alcohol in which the index moves, and the rest of the tube contains alcohol vapour only. The bulb is made cylindrical for several reasons; the chief being to secure uniformity of thickness, which is practically unattainable

(or at least unverifiable) in a sphere. By properly choosing the *thickness* of the cylinder in proportion to its bore, the sensitiveness of this gauge may be made as great or as small as we please. And, by having two or more, with bulbs of nearly the same *internal* dimensions, but differing considerably from one another in thickness of the cylindrical walls, a very important advantage is secured. For, under the same pressure, the maximum amounts of distortion of the glass are greater in the thinner bulbs, and thus these begin to deviate from Hooke's Law at pressures under which the thicker ones are still following it accurately. Thus, by comparison, we can easily find through what portion of its range each instrument gives effects strictly proportional to the pressure. The thinnest of these is to be graduated by comparison with the nitrogen gauge.

When this method has to be extended to pressures such as would crush glass, recourse must be had to steel, and a series of instruments with different thicknesses of this material is to be prepared. I do not yet know whether it may be found practicable to furnish these steel bulbs with thick glass tubes of small bore—probably we may succeed, if the steel be made to project into the glass. But if not, it is easy to construct them entirely of steel, so as to act on the principle of the "weight-thermometer." Anyhow, they can be graduated accurately from one another, each from a thinner one; until we come to the thinnest, which is to be exactly graduated by comparison with one of the thicker of the glass instruments. We have thus a series of gauges, each of any desired sensitiveness, capable of reading accurately pressures up to those for which steel at the interior of a thick tube ceases to follow Hooke's Law.

To illustrate this process, and to show what amount of sensitiveness is to be expected from an instrument of known dimensions, I append an approximate solution of the problem of the compression of a cylindrical tube with rounded ends. The exact solution would be very difficult to obtain, and would certainly not repay the trouble of seeking it. I content myself, therefore, with the assumption that all transverse sections are similarly distorted, which, of course, involves their continuing to be transverse sections.

Let ξ denote the displacement of a transverse section originally distant x from one end, and let ρ be the change of r the original distance of any point of the section from the axis. Then, as it is

obvious that the principal tractions are along a radius, parallel to the axis, and in a direction perpendicular to each of these, we have at once (Thomson and Tait, *Nat. Phil.* §§ 682, 683)

$$\frac{d\rho}{dr} = et_1 - ft_2 - ft_3,$$

$$\frac{\rho}{r} = -ft_1 + et_2 - ft_3,$$

$$\frac{d\xi}{dx} = -ft_1 - ft_2 + et_3,$$

where $$e = \frac{1}{3n} + \frac{1}{9k}, \qquad f = \frac{1}{6n} - \frac{1}{9k}.$$

Here $\frac{1}{k}$ is the compressibility, and n the rigidity.

In addition we have for the equilibrium of an element bounded by concentric cylinders, planes through the axis, and planes perpendicular to it,

$$t_2 = \frac{d}{dr}(rt_1);$$

and the approximate assumption above gives

$$\frac{d\xi}{dx} = \text{constant}.$$

From these five equations t_1, t_2, t_3, ρ, and ξ are to be found.

They show that t_3 is constant, and its value must therefore be

$$-\Pi\frac{a_1^2}{a_1^2 - a_0^2}.$$

With the surface conditions,

$$t_1 = -\Pi \text{ when } r = a_1,$$
$$t_1 = 0 \quad \text{,,} \quad r = a_0,$$

we determine the arbitary constants, and it is easy to see that

$$\frac{\rho}{r} = -\Pi\frac{a_1^2}{a_1^2 - a_0^2}\left(e - 2f + \frac{a_0^2}{r^2}(e+f)\right)$$

$$\frac{d\xi}{dx} = -\Pi\frac{a_1^2}{a_1^2 - a_0^2}(e - 2f).$$

Thus the diminution per unit volume of the interior of the cylinder is

$$-2\left(\frac{\rho}{r}\right)_{a_0}-\frac{d\xi}{dx}=\Pi\frac{a_1^2}{a_1^2-a_0^2}(5e-4f)=\Pi\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{n}+\frac{1}{k}\right).$$

When Π is a ton-weight per square inch, the value of the quantity

$$\Pi\left(\frac{1}{n}+\frac{1}{k}\right)$$

is somewhere about $\frac{1}{1000}$ for ordinary specimens of flint glass, and about $\frac{1}{4000}$ for steel.

It is obvious from the formulæ above, from which we have

$$\frac{d\rho}{dr}=-\Pi\frac{a_1^2}{a_1^2-a_0^2}\left(e-2f-\frac{a_0^2}{r^2}(e+f)\right)$$

that the greatest of the three compressions is that perpendicular to planes through the axis, while the least is radial. The former has its maximum

$$\frac{\Pi a_1^2}{a_1^2-a_0^2}(2e-f)=\frac{\Pi a_1^2}{a_1^2-a_0^2}\left(\frac{1}{2n}+\frac{1}{3k}\right)$$

at the inner surface where, therefore, the tube will first yield to crushing; the latter has its maximum at the outside. Their sum is constant.

If we compare two tubes with the same internal bore, 5$^{\text{mm}}$, but one two millimetres thick while the other is only half a millimetre, the maximum distortions under the same pressure are as $\frac{81}{56}$ to $\frac{36}{11}$ or 4:9 nearly.

When the pressure is internal we have

$$\frac{\rho}{r}=\frac{\Pi a_0^2}{a_1^2-a_0^2}\left(\frac{1}{3k}+\frac{a_1^2}{r^2}\frac{1}{2n}\right),\quad \frac{d\xi}{dx}=\frac{\Pi a_0^2}{a_1^2-a_0^2}\frac{1}{3k};$$

and the increase per unit volume of the interior is

$$\frac{\Pi a_0^2}{a_1^2-a_0^2}\left(\frac{1}{k}+\frac{a_1^2}{a_0^2}\frac{1}{n}\right).$$

In very thick tubes of narrow bore this is roughly $\frac{\Pi}{n}$, the value of which in glass is about $\frac{1}{1000}$ only for one ton pressure.

When the pressure is the same outside and inside the cylinder, we have

$$\frac{\rho}{r} = -\frac{\Pi}{3k}, \quad \frac{d\xi}{dx} = -\frac{\Pi}{3k},$$

and the diminution per unit volume of the interior is, as in Örsted's experiment,

$$\frac{\Pi}{k}.$$

For a spherical bulb the equations are reduced to

$$\frac{d\rho}{dr} = et_1 - 2ft_2,$$

$$\frac{\rho}{r} = -ft_1 + (e - f)t_2,$$

$$2rt_2 = \frac{d}{dr}(r^2 t_1).$$

and we have for external pressure Π,

$$\frac{\rho}{r} = -\Pi \frac{a_1^3}{a_1^3 - a_0^3}\left(\frac{1}{3k} + \frac{a_0^3}{r^3}\frac{1}{4n}\right).$$

As an external indication of the pressure (to guard against carrying it too far), a cylindrical steel bulb, screwed inside the compression apparatus, and filled with mercury, is used. Its indications are read by a glass tube inserted in its neck, which opens outwards.

I hope by means of the apparatus I have described to be able to measure approximately the volumes of gases and liquids at pressures amounting to 15 or perhaps 20 tons on the square inch. The only difficulty in the case is that at these high pressures the compressibility of such bodies is probably of the same order of small quantities as that of solids.

[*Added during printing.*—To diminish the effect of temperature on these instruments, the interior of the cylindrical bulb is now *nearly filled* by a glass tube sealed at both ends. Thus the quantity of liquid in the bulb is not much greater than that in the stem.]

4. Sixth Report of the Boulder Committee.

(Plates XVII., XVIII., XIX.)

The materials for this Report have been obtained from the Convener, Professor Forster Heddle of St Andrews University, William Jolly, Esq., H.M. Inspector of Schools, Inverness, and William Wallace, Esq., High School, Inverness.

To make the descriptions of the boulders more intelligible, it has been found necessary, as in former Reports, to annex a few diagrams, which will be found at the close of the Report.

I. *NOTES BY CONVENER.*

Argyleshire.

1. In consequence of information in the schedule issued by the Committee, and filled up by Mr Montgomery, schoolmaster at *Southend*, near Campbelton, I went to Southend, and had pointed out by Mr Montgomery boulders at several places there.

Mr Montgomery considered these boulders to be granite. If granite, they were different from any I had ever before met with. They were certainly an igneous or primitive rock of some kind, and composed of different ingredients, the chemical nature of which I am unable to state. There appeared to be crystals of white felspar. I could detect no mica. The general mass was a whitish-grey colour.

These boulders were pointed out to me at many places. I saw two in the River Conn (about half a ton in weight), one on Pennyserach farm (3 or 4 tons), another on the adjoining farm of Brunerican (1½ ton), another at Macherioch (about half a ton in weight). I heard of many more lying on the sea-shore adjoining these farms.

I was assured by Mr Montgomery, and by the tenants of these respective farms, that there was no rock in the south end of Cantyre similar to that of these boulders.

Along the east coast between Campbelton and Southend, a distance of 8 or 10 miles, I descried many boulders of the same nature; and in a gravel pit near Campbelton gas-work, I saw the fragments of another, which had weighed probably 3 or 4 tons.

In this same gravel pit I found a boulder of grey porphyry, con-

taining crystals of white felspar, somewhat similar to the Southend boulders. The gas manager informed me that he believed there was rock of the same nature a few miles to the north.

In the valley of Brackerie I found rock of a crystalline nature somewhat similar in composition to the boulders above described; and at a place in the same valley, called Collielangart, I saw a monumental pillar, about 8 feet high, similar in composition, said to have been obtained from Glenlissa, a place about 3 miles to the N.W. of Campbelton. I was informed also that boulders of this same rock, weighing 2 or 3 tons, had lately been observed in a recent cutting into boulder-clay to the north of Campbelton.

Professor Nicol, in a short account of the Geology of Cantyre in the "London Geological Society's Journal" for 1852 (vol. viii. p. 421), refers to the boulders at and near Southend. He describes them as *white granite*, and as resembling a granite in Arran, from which, therefore, he supposed these Southend boulders had somehow been transported. Professor Nicol takes notice of several striated rocks on the east coast of Cantyre, one of which showed a direction of E. 10° N. by compass, which he remarks is nearly parallel to the line of coast, and in the direction of Arran, 25 miles distant.*

There was only one spot where I found a smoothed rock, viz., about a mile to east of Campbelton. It sloped down to N.N.W. at an angle of 40°. There were no striæ.

I offer no positive opinion regarding the position of the parent rock from which these *white granite* boulders came. It is pretty clear, however, that those at Southend must have travelled from the north, and many of them there are lying on the Old Red Sandstone strata which fringe the south-west coast of Cantyre.

Another part of Cantyre visited by me was the district between Campbelton and the west coast at *Drumlenbie and Balahunty.*

Near *Kilhenzie*, a few miles west of Campbelton, there are hills reaching to a height of from 500 to 600 feet, well covered with detritus; and on their western slopes there are numerous boulders of

* With reference to Professor Nicol's view that the white granite boulders seen on the east and south coast of Cantyre came from parent rocks in Arran, it is right to notice that the late Rev. Mr M'Bride of Rothesay, who was a good geologist, and well acquainted with the rocks of the West Highlands, suggested a more northern source (Bryce "On Arran," 4th edition, p. 337).

gneiss and mica schist. I measured several, the largest contains about 150 tons.

The Old Red Sandstone formation occupies the west coast for some miles. It is well covered by detritus, and on the detritus, especially when it slopes to the west, there are many boulders of granite and gneiss, which from their position appear to have come from the N.W.

Fig. 1, plate XVII., represents a bank of gravel, at a height of about 50 feet above the sea, and sloping to the sea in a N.N.W. direction at an angle of about 25°, well covered with gneiss boulders, of which three are represented. There was no rocky cliff from which they could have fallen. They were true erratics. Thick turf had formed on the bank, which partly covered the boulders.

Fig. 2, plate XVII., represents, near the above spot, another boulder of gneiss, at a height of about 40 feet above the sea, lying on a mass of reddish coloured mica schist, blocked at its south end. Its longer axis lay N. by E. and S. by W. It had apparently come from the north, and been stopped in its further progress by the rocks at its south end.

On the shore, I found a boulder partially in a fissure which cuts through the mica schist strata, here forming tablets or sheets nearly horizontal. Figs. 3 and 4, plate XVII., represent a fissure running N.W. and S.E., about 6 feet wide. The boulder, very hard gneiss, 10 × 5 × 4 feet, was sticking in this fissure. Fig. 3 represents the fissure running N.W. and S.E., and the upper part of the boulder projecting above it. Fig. 4 represents the interior of the fissure partially filled with pebbles, and the boulder resting partly on them and partly on the S.W. wall of the fissure, whilst the other end of the boulder, outside and above, was resting on the N.E. edge. The boulder had clearly been pushed from the northward (from B in figs. 3 and 4), and on reaching the fissure had partially fallen into it, and become jammed there.

If the idea of a sea-current from the north, with ice floating in it, be entertained, there seems to be no difficulty in explaining the above facts. The weight of the boulder was about 15 tons.

On the same part of the shore, there were many other hard gneiss boulders. The largest measured was 12 × 6 × 6 feet, its longer axis pointing north and south.

Specimens of these boulders, found by me in Cantyre, I submitted to Professor Heddle of St Andrews University, so well known for his acquaintance with the igneous rocks of Scotland, and their mineralogical composition. He has kindly supplied the following notes:—

(1) Most of the Southend boulders, and those along the east coast between Campbelton and Southend, are identical in composition with one variety of the coarse porphyritic rock of Davar Island, situated at the mouth of Campbelton Bay.

(2) One specimen is a small-grained white granite, which I think I have seen somewhere in Arran.

(3) One specimen from the west coast is a coarse grey granite, identical in appearance with the granite of the Mourne Mountains in the N.E. of Ireland. I observe in this specimen two crystals of topaz. This granite, from containing also crystals of albite and of a lithian mica, should be easily recognised.

2. *Loch Lomond.*—On the west side of the lake, near Arden, a lateral valley runs up towards the west. There is a horizontal terrace in this valley about 70 feet above the lake, bounded by a steep bank, showing that at one time the lake had filled the valley up to that height. On this flat lie a number of quartz, granite, and mica schist boulders, which most probably all came from the westward, as the rocks in the valley are Old Red Sandstone. The head of the valley reaches to about 150 feet above the sea. The land then slopes down westward towards the sea in Loch Long. If these boulders were floated from the westward, it must have been when the sea was at a greatly higher level than 150 feet. The largest of these boulders, a mica schist, I found to be 5 × 3 × 3 feet, with its longer axis lying E. and W., and its sharpest end towards the west.

On the east bank of the loch, nearly opposite to Arden, on the farm of Over Balloch, and at a height of about 337 feet above the sea, I found a grey granite boulder, 5 × 4 × 4 feet, much rounded. Its longer axis lay in like manner E. and W. It was on a bed of boulder-clay. It most probably had come from the west or north-west, crossing therefore the valley now occupied by Loch Lomond.

3. *Loch Long and Gareloch.*—On the ridge between Gareloch and Loch Long I found several boulders. At a height of 160 feet above

the sea, there is one of mica slate, 11 × 6 × 6 feet, lying on rocks of clay slate. Its longer axis is N. by E. and S. by W., its sharpest end being to the north. The axis was parallel to the valley of Loch Long. Its south end was pressing against a knoll of gravel as shown on fig. 7, plate XIX., which seemed to have intercepted its farther progress to the south. This boulder had apparently come down Loch Long, though whether floated by ice or carried by a glacier, is a question. But the knoll of water-borne gravel at its south end, favours the former theory.

Another boulder on this same ridge, 8 × 6 × 5 feet, occurs at about 360 feet above the sea, also blocked at its south end by a rocky knoll.

In the Gareloch, on the east side, a little below Shandon, on the beach, a gneiss boulder occurs, 18 × 15 × 12 feet, with its sharp end pointing N.N.W. The boulder on that side presented a very smooth surface. Every other side was rough.

The foregoing boulders as regards *position*, may all be accounted for by the supposition of the transporting agent having passed through the valleys in which they are situated, in a southerly direction.

The boulders in the Gareloch and Loch Long were reported on by the late Charles Maclaren, and the opinion which he formed was that they had been brought to their present position by floating ice.

It appears that the late Sir Roderick Murchison visited this district, and gave an opinion against the theory of glaciers as applicable here.—(Chambers in "Edinburgh New. Phil. Journ.," vol. lv.)

4. *Loch Fyne.*—I was invited by Mr M'Killop, schoolmaster at *Loch Gair*, situated about 7 or 8 miles west of Inveraray, to inspect some large boulders in that district.

The first block seen was situated about 3 miles to the north of Loch Fyne, surrounded by hills, most of them covered by drift. It was 23 × 17 × 12 feet, its longer axis lying N.N.E. and S.S.W. It was resting on a knoll of gravel, and at some distance from any hills. It was clearly an erratic,—a coarse gneiss. At first I was puzzled to account for its position being so exactly on the apex of the gravel knoll. It struck me eventually, that its great size and weight had been the means of protecting, by covering the knoll on which it originally had been dropped. The denuding agencies which could loosen and sweep away the gravel and sand in the surrounding parts of the

valley, probably did not move the boulder, and so would leave it in its original position or nearly so.

I proceeded next towards *Loch Glashan*, and was rather surprised to see the hills on its south side, which sloped down towards the N.E., well-covered with boulders, and also striated rocks, facing N.E. and N.N.E. In fig. 5, plate XVII., there is a view of one of those boulders, $8 \times 4 \times 3$ feet, on Knock farm, resting on a smoothed rock, dipping N.N.E., at an angle of about 30°, and at a height of about 400 feet above the sea. At this place, looking towards the N.N.E., there seemed to be a sort of low level district for some miles, with high hills on each side. On examining the map, I found that Loch Awe and Loch Etive were in that direction.

5. A few weeks after being at Loch Gair, I visited *Loch Awe*, and remained for a few days at *Port Sonnachan*, situated on the south bank of the lake.

On inquiring of the innkeeper, I was informed of a remarkable boulder situated among the hills to the south, and distant 3 or 4 miles. Having obtained the services of a shepherd as guide, I proceeded on foot across the moors, and came to a high corry, with a ridge in the middle, on which ridge the boulder stands at a height of 1026 feet above the sea. The boulder is so distinguishable from every other in the district, that the corry takes its name from it, viz., *Corry na clach*.

Fig. 7, plate XVII., gives a distant view of the boulder among the hills to the south of Loch Awe. Fig. 8 shows its position on the ridge where it stands. This ridge is narrow and has steep sides, so steep that they can be climbed with difficulty. The side facing the south is about 80 feet, the side facing the north is about 50 feet, in height, above the level ground adjoining.

The ridge, shown in figs. 7 and 8, is composed entirely of a soft arenaceous mica schist, in thin slaty strata, which stand up vertically, and form a table about 3 feet above the ridge, as shown in fig. 9. This table, on which the boulder sits, is about 5 feet square. The surface of the table slopes down to W. by S. at an angle of 22°. The boulder on this table of rock occupies a most precarious position. Stooping below the boulder, to examine the strata forming the table, I saw daylight across, under the boulder, and observed

that it touched the rocky table at three points, each point of contact consisting of a few square inches.

The boulder was 13 feet long, 12 feet wide, and about 6 feet high. Its longer axis lay across the ridge, viz., about N. and S. Neither the nature of the rock composing the boulder, nor its own position, gave any indication of the direction from which it had come. It was a hard compact gneiss, the rock which prevails in most of the hills of the district on all sides. One feature in the position of the boulder offered a suggestion, though slight, as to the direction of its transport. If the rocky table on which it lay was sloping as now (at an angle of 22° to the west) when the boulder landed on the table, it is probable that it must have come from a westerly rather than from any other point. If it came from an easterly direction, it would, by its own weight when still in motion, have slid off the table altogether. But the assumption that the table on which it rests was originally sloping as now, may not be correct. On this ridge denudation may have changed the surface—except where protected by the boulder. Moreover, it is possible that the boulder itself, by the mere action of the wind upon it, might cause it to move on and abrade the rock. The space between it and the rock may also have been acted on by frost. Certain it is, that at present the stability of the boulder is most precarious. With a lever, I could easily have moved the boulder off its site. The innkeeper at Port Sonnachan informed me that there had actually been a proposal by some travellers staying at his inn, to perform this exploit, and that he had prevented it.

I am unable to explain how the boulder could have got on the apex of the hill, except on the supposition that a sheet of thick ice, strong enough to float the boulder, may have stranded on the hill; and that when it melted, the boulder might have subsided on the part where the ice had stuck.

6. Having asked my guide, whether there were any other large boulders in the neighbourhood, I was conducted by him to the side of a hill, about $\frac{1}{4}$ of a mile to the eastward, well-covered with boulders. The height above the sea was about 900 feet. I was rather surprised to find the boulders here in such positions as to indicate that they had come from N.N.E. The largest measured 18 × 10 × 10 feet, and its longer axis lay N. and S. I observed that most of the other boulders lay in a similar position. The rocks presented smoothings

which faced N.N.E., being the direction in which Loch Etive lies.

I remembered that in walking up from Loch Awe on this occasion, I had seen several smoothed rocks with striæ running much in the same direction, but I omitted to take the exact bearings.

I felt surprised at this direction, as, when last year in the Hebrides and the west coast of Argyleshire, I had been accustomed to see that N.W. was the usual direction both of boulder transport and of rock striations.

7. This N.N.E. direction of transport appears, however, to characterise all the boulders and the rock striation at the *Gareloch*, *Loch Gair*, and *Loch Awe.* It will be observed that these places form a band or line across the country about N.N.E. and S.S.W. It is no doubt premature to theorise on so small a number of facts recorded in these notes. But they seem to suggest that in this district there may have been a current of floating ice, moving in a S.S.W. direction, dropping boulders where the ice which bore them was stranded or obstructed.

Is it not probable that, when the Highlands of Scotland were covered by the sea, up to a height of say 2000 feet, and when they presented an archipelago of islands, there may have been currents moving in different directions, and these directions changing as the sea fell from one level to another?

The valley through which the Highland Railway passes, between *Killin and Dalmally*, presents, on the sides facing and sloping down to the north, many examples of large boulders and striated rocks, which, even from a railway carriage, are seen to be well-deserving of special investigation. Thus at *Luib* station, and for some miles both to the east and west of it, there are numerous large boulders resting on the hill sides sloping down to the north; as also great masses of boulder-clay and water-borne gravel, with huge boulders, and occasionally under these beds, surfaces of rock, well smoothed and striated. A special examination of this district would be rewarded by many important discoveries. Similar features occur at and near *Crianlarich* and *Tyndrum.* But at Tyndrum, while there are knolls and escars of gravel, so numerous indeed that they have given a name to the place (in Gaelic),* there is a sudden

* *Tigh*, dwelling; *Drum*, ridge or back.

and remarkable cessation of boulders. This absence of boulders continues west of Tyndrum till about 2 or 3 miles east of Dalmally, when they again begin to make their appearance, and they are very numerous on the hills there facing the N.W.

May the reason of this be, that at or near Tyndrum there is the valley (traversed by the high road) running in a N.N.W. direction between high mountains, passing through Glencoe, whilst near Dalmally there is a similar opening towards the sea by Loch Awe and Loch Etive. When the sea stood at say 2000 feet above its present level, currents may have flowed through both of the openings just described, but not over the high ground between Dalmally and Tyndrum, the land there being so high that it may have prevented a current. It will be remembered that in the Committee's Fifth Report facts were stated, which seemed to show that in Glencoe a current had passed up the glen carrying boulders towards the S.E.

The current which passed through what is now Loch Etive and across Loch Awe, towards the S.S.W., may have continued till the sea sank below the level of the hills lying in that quarter. Along the banks of Loch Awe there are sea-terraces at a height of at least 200 feet above the present sea-level; and in the narrow pass at the south end of the loch, near Ford, there are indications of a current which flowed through it from the north.

Whilst on the subject of Loch Awe, I may notice a boulder on the south bank of the loch, at a place called Kaim, about 10 miles west of Port Sonnachan. The boulder is of mica schist, and is $24 \times 11 \times 9$ feet. Its longer axis is N.W. by N. It rests on a knoll of gravel which is about 20 feet above the adjoining meadow. This meadow is surrounded by hills (from 400 to 600 feet in height) on all sides but one, where there is an opening due west from the boulder, and by this opening the boulder may have entered the *cul de sac* where it lies, though, if brought when the sea was 400 to 600 feet higher than now, it may have come from any other direction.

The hills to the south of this meadow are, on their sides sloping down to the north, well-covered by boulders; and they apparently had come from some northerly point.

Along the south bank of Loch Awe, between Port Sonnachan

and Kaim, striated rocks occur, almost all of which present surfaces towards the N.W. Fig. 8, plate XIX., gives an example of one of these rocks. It slopes down N. by W. at an angle from 60° to 70°. The striæ upon it run E. and W., dipping down east at an angle of 20°. In one part of the surface there is a hollow, A, over which the striating agent has passed without marking its sides. The striating agent had moved from the west, as the striæ were deepest at their west ends.

8. *Ardrishaig.*—This place is on Loch Gilp, a small branch from Loch Fyne. There are hills here on the west side of Loch Gilp which rise to a height of from 700 to 800 feet. Fig. 1, plate XVIII., gives a bird's-eye sketch of the loch and these hills. No. 1 is the town of Ardrishaig. No. 2 is Loch Gilphead. A lateral valley comes down from the S.W. marked AB.

At *a* and *b* many of the rocks (a sort of clay slate) are (at a height of 420 feet above the sea) well smoothed, the smooth faces being parallel with the axis of the loch, which here runs about N. and S. The smoothing had evidently come from the north. On reaching *cd*, at a height of 600 feet above the sea, the smoothed rocks were more abundant, and evidently from the same direction.

A Boulder of dark-coloured granite, 12 × 6 × 4 feet, was found between *ab* and *cd*, whose position implied transport from the north.

At a height of from 500 to 600 feet between *a* and *c* there are numerous whinstone knolls, sloping down towards the north as indicated on fig. 2, plate XVIII., beautifully smoothed and polished on the north sides, but rough and precipitous on the south sides. On some of them a few boulders were lying, evidently intercepted in their farther progress southward by these knolls, because on the south side there were numbers of boulders which, having been pushed over, had remained there, as shown in the figure.

At one place indicated on the sketch, fig. 1, plate XVIII., by the letter *e*, a smoothed rock was met with sloping down S.E. by S. at an angle of 35°. Two sets of striæ were on it, one running N.W. by W., the other running N.E. and S.W.

This spot was very near the corner where the lateral valley AB joins the main valley of Loch Gilp. The two sets of striæ indicated of course two several currents, one apparently parallel with the axis

of the lateral valley AB, and the other nearly parallel with Loch Gilp valley.

At the mouth of this lateral valley, at *f* on the sketch, a number of boulders were found at a height of about 670 feet above the sea, lying on the hill slopes facing the N.W. Some of these were in such positions as showed transport from the N.W. One example is given in fig. 3, plate XVIII., where a boulder A, 4 feet high by $2\frac{1}{2}$ feet wide, was resting against the N.W. sides of rocks B, on their W.N.W. sides.

On proceeding to the head of this lateral valley, about 1 mile distant, and at a height of about 795 feet above the sea, I found numerous boulders, many of large size, resting chiefly on rocks and hill slopes, facing W.N.W., and with their longer axis lying much in the same direction. One of these measured $12 \times 6 \times 5$ feet.

There were several smoothed but no striated rocks. At one place, however, at a height of 650 feet above the sea, I found a mass of softish Silurian rocks traversed by a hard quartz vein about 2 inches thick, standing up above the Silurians, as shown by A in fig. 4, plate XVIII. This vein had been evidently ground down by something which had passed over it from W.N.W. The quartz retained a beautiful polish, but the Silurian rock, though it had once presented a smooth flat surface, had become rough by atmospheric action. Being softer than the quartz vein, it had been ground down more effectually by the agent which had passed over.

9. *Loch Killesport.*—Having been informed by Mr J. F. Campbell of Islay (author of "Frost and Fire") that the largest boulder he had seen in Scotland was on the south side of *Loch Killesport*, near Ormsary House, I went there in company with Mr Alexander of Lochgilphead, who was so obliging as to undertake to be guide. His local knowledge was of much service to me.

We went first to some boulders a little beyond Ormsary House on the sea-shore. One (of gneiss) had a girth of 65 feet and a height of 16 feet. It was tolerably well rounded, its sharpest end pointing N.W. Another measured $17 \times 13 \times 5$ feet, its longer axis lying N.W. by W. Another measured $24 \times 12 \times 5$ feet, its sharpest end pointing W.S.W.

The largest boulder was situated about $\frac{1}{4}$ of a mile east of Ormsary House, on the south side of the coast road, adjoining a ruinous

cottage. The boulder was in two pieces, having evidently been broken by some natural agency. Before it broke, it must have measured in length 52 feet, in width 36 feet, and in height 20 feet, containing 1387 cubic yards or about 2770 tons.* It was extremely angular in shape. Its narrowest end was to the west. This immense boulder was at the foot of what was evidently an old sea-bank, whose base is about 40 feet above the sea at high water. The bank is about 35 feet in height, and consists chiefly of gravel and boulders.

Fig. 5 on plate XVIII. is intended as a sketch from memory of this spot, AAA being the old sea-bank, B the large boulder, and CD the high road along the south shore of Loch Killesport.

Along the line of this old sea-bank, there were great numbers of boulders above it and at its base. The measurements of a few may be given. A boulder at base of the bank, measured 23 × 14, lying E. and W.; another measuring 20 × 12 × 10 feet, lay on the slope of the gravel bank, which here faces W.S.W.

Another boulder on top of bank measured 24 × 16 × 13 feet; and another 18 × 10 × 8 feet, lying on gravel with its sharp end to the W.N.W. There were about twenty more, smaller than these, scattered on the fields above the old sea-bank.

At a little distance from the top of the bank, I found a rock of mica schist well smoothed, with striæ running E. by N. and W. by S.

Near the village of Ballibayach, in a field to the north-east, there is a gneiss boulder, 33 × 18 × 12 feet, resting on a smoothed rock of mica slate, which slopes down towards the west. This boulder weighed probably above 400 tons. Along the range of hills and up to their summits, at a height of about 600 feet above the sea on the south side of Loch Killesport, numerous boulders were seen from the road; but I was prevented going to them.

About 3 miles to the east of Ormsary, the road passes through the narrow valley of Auchloss, which runs in a direction about east and west. Smoothed rocks occur in this valley, on one of which Mr Alexander pointed out striæ running in a direction W. by N.

* In the "American Journal of Science and Arts" for 1877, reference is made to a boulder in Vermont, called "The Green Mountain Giant," weighing about 3400 tons; and to twelve still larger in New Hampshire—the largest measuring 62 × 50 × 40 feet, and estimated to weigh nearly 6000 tons.

These striæ seemed deeper at their west ends, as if the tools which cut them, had struck the rock first at these ends.

The boulders on Loch Killesport appeared to me, from their positions, to have all come from the westward. If they came on floating ice, the sea must at that time have stood at a high level to have floated ice of sufficient size to carry and deposit boulders of such weight as those above described. On that point there need be no difficulty, as there is abundant evidence that the sea prevailed over the Highlands of Scotland to a height of at least 2000 feet, and thereafter subsided, whether gradually or rapidly is not yet known. The sea-bottom on which the boulders were dropped, would of course present a very different surface from what forms the present dry land. What are now valleys in the land would be formed (after the sea subsided) by the detritus which filled these hollows being scoured out by rivers; whilst the boulders which had occupied the old sea-bed, when too heavy to be moved by river floods, would remain in the newly formed valley, though sometimes at lower levels. In like manner, the boulders which are now on the shores of sea lochs, may in many cases have been undermined by the scouring out of detritus by tides and storms, and sunk to a lower level than they originally occupied. Hence it is that along the *present* line of high water the boulders are generally more numerous than elsewhere; and the same circumstance occurs everywhere along the *old* sea-margin, as in Loch Killesport.

10. Another place visited was *Loch Swin*, an arm of the sea on the west coast of Argyleshire, about 16 miles west from Lochgilphead. Mr Alexander of Lochgilphead kindly accompanied me to this district also. At *Keills*, on the north bank of the loch, close to its mouth, there are several boulders of a light-coloured grey gneiss, and one or two of a fine-grained granite. The rocks on which they rest are a coarse dark-coloured Silurian.

The first boulder examined was on the shore facing the island of Jura, here distant about 4 miles to the west. Its size was $12 \times 10 \times 9$ feet. It lay on a bank sloping down towards the sea at an angle of about 15° to the W.N.W. It rests on Silurian rock, at a height of about 50 feet above the sea, and about 100 yards from the beach.

About a mile to the eastward, and not far from the old ruinous church of Keills, there is another grey gneiss boulder, $18 \times 15 \times 12$

feet, resting on a terrace about 150 feet above the sea. Another boulder, of fine-grained granite, lies near it on the same terrace.

Another boulder is on the shore here, 16 × 10 × 9 feet, with the longer axis lying E. and W. I learnt from Angus, shepherd at Keills (and who also acts as post messenger), that on the *Island of Dana*, at the south side of Loch Swin, there are three boulders larger in size than any on the north side.

On my return to Lochgilphead, I walked to *Carig Bay*, in the parish of North Knapdale, and on the hill to the S.W. facing the island of Jura was shown a fine-grained gneiss boulder, 12 × 6 × 6 feet, similar in composition to those at Keills. It was resting on a rocky surface sloping down to N.W. Its position suggested transport from N.N.W. Its height above the sea was 270 feet.

At *Tayvallich*, on the north shore of Loch Swin, I fell in with boulders forming two groups of 3 and 4 in number, whose relative position indicated transport of the uppermost from the west.

At *Scotnish*, also on Loch Swin, found an old sea-terrace at 42 feet above the sea, with a boulder on it, 18 × 11 × 8 feet, besides many others of smaller size.

In a short lateral valley opening into Loch Swin at *Loch Mhurrich*, I had shown to me by Dr J. M'Leod of Tayvallich an immense boulder, 36 × 15 × 13 feet, weighing about 500 tons. It rests on a knoll of shingle, and is about 30 feet above the sea-level, and distant from it about $\frac{1}{5}$th of a mile. This knoll is in the centre of a marshy meadow, which is surrounded by hills of from 260 to 300 feet in height, whose sides show beds of sand and gravel. The mouth of this small loch opens on Loch Swin to the W.S.W. The boulder is many hundred yards distant from the adjoining hills, so that there is no doubt that it is an erratic. But from what quarter, and by what means has it come? One naturally supposes that it must have come in by the mouth of the valley, of course at a time when the sea was deep enough to float it and lodge it in this *cul de sac*. The sea-bottom on which it dropped may then have been higher than the existing meadow; and as the detritus was washed away, the boulder may have protected the bed on which it rested, so as to form the present knoll.

I may add that there are numerous small lateral valleys along the north side of Loch Swin, extending a few hundred yards, and run-

ning in a N.E. and S.W. direction. They open into Loch Swin, and are well filled by boulders. These are generally most abundant on the south sides, and on the slopes of hills facing towards the N.W.

Berwickshire.

1. In *Ayton* parish, on Whitfield farm, 2 or 3 miles N.E. of the village, several small boulders of grey granite occur, about 270 feet above the sea. Nearest rock of same kind is on Cockburn Law, about 10 miles to W.N.W. Near Ayton Castle a bed of sand was excavated to the depth of about 20 feet and removed. Bits of coal (including cannel) were found in the bed, about 200 feet above the sea. Nearest place where coal strata occur, is in Mid-Lothian, on north side of Lammermuir Hills, about 30 miles to N.W.

2. In *Coldingham* parish, on Crosslaw farm, well rounded masses of hematite were turned up by the plough at a height of about 500 feet above the sea. Nearest place where hematite has been found is in East Lothian, about 30 miles to N.W. But the range of Lammermuir Hills intervenes. On this same farm, a boulder of white coal sandstone occurs, which must have come from East Lothian. Lumps of coal have also been found there in the boulder clay on Blackhill farm.

"On the heights east of Coldingham Loch, the rocks lie in separate and parallel ridges. The ridges are much rubbed and planed, especially on the N.W. exposures, as if some mighty force had battered and grated them down. There were also indications of striæ, which bore by compass nearly north, or N.½-W.—in this agreeing exactly with the striæ at St Abb's Head and the Farne Islands."—(*Address by Jas. Scott Robson, President of Berwickshire Nat. Club*, vol. vii. p. 175.)

3. In *Chirnside* parish, at Old Castles, there are numerous boulders of grey granite, from 1 to 2 tons in weight, and about 300 feet above the sea. Nearest rock of same kind is at Stanchal and Cockburn Law hills, visible from Old Castles, and about 8 miles distant to N.W.

4. At *Blackadder*, in Edrom parish, a boulder of blue whin or greenstone is on a knoll of gravel, on the west side of knoll. Its

height above ground is about 4 feet, and its width 2 feet. Level above the sea about 250 feet. Nearest rocks of same kind are at Hardens, about 5 miles to N.W., and about 500 feet above sea.

5. In *Hutton* parish, at Paxton brick-work, buried in boulder clay, a blue whinstone boulder, $7\frac{1}{2} \times 4\frac{1}{2} \times 3$ feet, weighing about 12 tons, was found. Its longer axis pointed N.W. by N. In that direction, Borthwick Hill near Dunse is situated, distant about 10 miles. It is the nearest spot for whinstone rock *in situ*. In the same brick-work, small boulders were found of old conglomerate, greywacke, chert, and white sandstone. Rocks of these kinds occur in Berwickshire to the westward. The brick-coloured porphyries of Kyles and Dirrington hills, situated about 15 miles to the west, were found there also.

Blocks of the same blue whinstone occur on the farms of Broadmeadows and Sunwick. Blocks of a peculiar greywacke, of a concretionary character and black in colour, occur in the Pistol plantations. The only rock of that kind known to exist is in the channel of the Whitadder, near Cockburn Law, about 14 miles distant to the N.W. Blocks of the same peculiar rock occur in great numbers on all the farms in the same line. There are specimens of them at Paxton House.

6. At *Stitchell Craggs*, pebbles of Old Red Sandstone are lying on the whinstone rocks, at a height of about 600 feet above the sea. Nearest place where Red Sandstone strata occur is some miles to the west. On the west sides of these craggs there are smoothed surfaces of whinstone rock dipping towards W.N.W. None are seen on any other side. At *Baillie Knowe*, in same parish, about 300 feet above the sea, a whinstone hill occurs, presenting on its west side similar smoothed portions of rock dipping W.N.W.

Cowdenknowes Hill, situated in Earlston parish, consists of felspar porphyry. Large blocks of this rock are strewed over the muirs situated to the east, resting on Old Red Sandstone strata.

On *Smailholm Craggs*, about 3 miles west of Stitchell, at a height of 570 feet above the sea, rocks facing W.N.W. show striæ made by some agent coming from W.S.W.

On *Hume Castle Craggs*, at height of about 740 feet above the sea, there are rocks smoothed and striated, in an E. and W. direction.

"Boulders, carried a hundred miles and more from their native localities, are still found in many parts of Berwickshire, though by far the greater number, especially of the smaller ones, have been broken up for road metal. This is particularly the case along the post road between Reston and Ayton, where fragments of gneiss, mica slate, pure vein quartz, porphyries, and other rocks of Grampian origin, were, a few years ago, to be seen in every depôt by the roadside. The current which brought the ice upon which these were conveyed, must have come from the westward, where these rocks occur *in situ.* Among the more remarkable of these boulders may be mentioned a rounded block of gneiss on the road at the top of Ecclaw Edge,—a large block of mica slate on the slope of the hill east from Burnhouses,—several smaller masses at Windshiel, Kidshielhaugh, and Abbey St Bathans,—and a block of a very peculiar diorite, formerly one of the stepping-stones in the River Whitadder at Ellenford. This diorite, which is composed of greyish quartz, red felspar, and a little chlorite, occurs *in situ* in the neighbourhood of Aberfoyle. Rounded pebbles of the same have been found in the Whitadder below Preston Bridge, where also mica slate, quartz, sandstone from the Lothians, &c., are to be met with in the river shingle."—(*Wm. Stevenson on Ice Action in Berwickshire,* "Berwick. Nat. Club Trans." vii. p. 209.)

Buteshire.

Arran.—Some years ago I spent a few days at Brodick and Corrie, and made the following observations:—

1. In travelling along the high road between Lamlash and Brodick, I observed thick beds of boulder-clay containing numerous boulders, the most prevalent being granite, and also a conglomerate, with large quartz pebbles in it. The height of these clay-beds was about 387 feet above the sea. Rocks *in situ* of the same nature are situated to the N.N.W.

To the south of Brodick Bay, there is a large number of Boulders, along and near the coast; but in Brodick Bay itself, there is a total absence of boulders, whilst to the north of Brodick Bay they are numerous.

This circumstance suggests a theory which will be mentioned, after

some account has been given of individual boulders remarkable for size or position.

One of the boulders to the south of Brodick Bay is known by the name of the Corriegill Boulder. It lies near the shore. Its highest point is about 15 feet above its base, and its girth is about 60 feet. Its shape is indicated by fig. 1, plate XIX., representing a section through it horizontally a little above the base. Its longer axis lies N.W. and S.E., with its sharpest end to N.W.

The boulder is granite of a grey colour, the ingredients being crystals of quartz, felspar, and mica, which are all rather larger than usual in size, and give to specimens a very coarse and rough aspect. It has a vein of finer grained granite running through it.

The top of the hill called Goatfell bears from this boulder N.N.W., and is distant about 4 miles. Granite occurs *in situ* on Goatfell.

Another boulder was measured, situated about half a mile to the north of the above on the shore at half-tide. It was 12 × 9 × 8 feet. Its longer axis lay due north and south.

From this part of the coast, where these boulders begin to be numerous, the northern horn of Brodick Bay, at the sea-shore, bears N. by E. This horn is a continuation of a steep ridge which runs up to Goatfell.

2. To the south of Corrie (about a mile) two boulders of considerable size are situated on a plateau or terrace, which is from 89 to 96 feet above the sea.

The largest is shown on fig. 2, plate XIX., A being a horizontal section near the base, to show dimensions of the sides and their position by compass; B indicates the position of the greatest mass which is at the south end, the highest point there being 15 feet above the base.

The longest axis is in a direction about N. by W. and S. by E.

I calculated the weight of this boulder to be about 620 tons.

I omitted to mark the nature of the rock composing these two boulders; but they are, according to my recollection, grey granite.

Goatfell from their position bears about W. by S., and is distant about 3 miles.

The rocks of the district where these boulders lie are sandstone,

apparently carboniferous. There is a quarry for building purposes not far off.

These two boulders must have been *carried*, for there are no adjoining hills from which they could have fallen. Carried by a glacier they could not be, as they are not in a valley nor near any from which a glacier could have issued.

3. To the north of Corrie, about 2 miles, the road passes a large boulder called the Catstane, which is about 18 feet in height and 56 feet in girth. The late Dr Bryce estimated its weight at above 200 tons. I calculated its cubical contents to be 131 yards, and therefore its weight about 262 tons. It is very angular in shape; but I could not ascertain correctly the length and direction of its different sides.

On the beach near the last-named boulder, there is a granite boulder which I was able to measure with exactness. It is in height about 12 feet. Its longer axis lay in a N. and S. direction, its narrowest end being to the north. Its shape and the length of its sides are shown in fig. 3, plate XIX. I estimated its cubical contents at 106 yards, and its tonnage about 212 tons.

The boulder next larger in size at the same place is shown in fig. 4, plate XIX., A and B, where A represents the lengths of the different sides, and B gives an idea of the height, which was about 10 feet. The direction of the longer axis and of the narrowest end was much the same as in the other boulder.

Another granite boulder on the shore (at the old sea-margin of 12 feet above the present sea-level) is shown on fig 5, plate XIX., where A gives a horizontal section to show its shape and direction of its longer axis, and B its peculiar position, resting as it does on a mass of Red Sandstone (coarse) conglomerate strata, rising up towards the north. The position of the boulder, blocked as it is at its south end by the sandstone, shows that it has come from the north. The girth of this boulder is about 33 feet, its length about 9 feet, its width 8 feet, and its height 8 feet.

Many blocks of the conglomerate sandstone on which this boulder rests are found along the shore to the south, none to the north. It will not fail to be observed that one feature characterises all the cases of boulders just mentioned. The narrowest end points towards the north, suggesting the idea that, after being deposited, they

had been subjected to some agency which put them into a position enabling them to withstand any farther dislocation.

That this agency came from the north, their position clearly indicates, an inference confirmed by the transference towards the south of the sandstone blocks above mentioned.

4. On ascending the hills to the west of Corrie, I found smoothed surfaces on the sandstone rocks and traces of striæ at a height of about 158 feet above the sea. The direction of the striæ was N.W. and S.E.

On these hills, up to a height of about 587 feet above the sea, the boulders are very numerous. All that I examined were of grey granite, except three, and these were of conglomerate.

Between these hills and the high granitic boss to the west, reaching to a height of about 1800 feet, there is a valley running N. and S.

The high hill on the other side of the valley to the westward is composed of grey granite. I climbed this hill up to a height of 1270 feet. The ground passed over was thickly strewed with grey granite blocks. I could here distinguish two sets of boulders—*one* set angular, which may have fallen from the mountain—*another* set well rounded, which seemed to be erratics, not only because of their shape, but because they were of a harder texture than the rock of the hill. The ingredient crystals were also larger in size. These rounded blocks I observed to be on the hill-side for at least 100 feet above the point reached by me.

One of the boulders arrested my attention on account of its size and position. It was 25 feet long, 9 feet wide, and 12 feet high. This boulder and many others were lying with their longer axis N. and S., and could not, as it seemed to me, have fallen from any rocks on the hill above.

5. I went across the island to Loch Ranza, the summit-level being about 660 feet above the sea.

I was unable to examine any individual boulders. But I noticed that there were many more on the east side of the summit-level than on the west side.

I saw on the hills facing the N.W. numerous "perched blocks," at heights of from 1600 to 2000 feet. But they were too far off to admit of examination.

Near Loch Ranza some remarkable terraces, with boulders, arrested my attention, at heights of from 80 to 100 feet above the present sea-level. Great scaurs of gravel and sand, which were in beds, sometimes flat, sometimes dipping at a high angle, were under these terraces.

6. In connection with the Arran boulders, reference may be made to the following:—

Ailsa Craig (Ayrshire) is a mass of trap,—much of it (as I understand from Professor Heddle) being columnar porphyry of a white colour. It reaches to a height of 1114 feet. Not having visited it myself, I may be allowed to refer to information given by others.

In a paper by Mr W. N. Macartney, in the "Proceedings of the Glasgow Nat. Hist. Society for 1868," it is stated that the Craig bears many marks of glaciation, up to near the top. On the north side there is, at the height of about 600 feet, a deposit of boulder-clay in a slight depression of the rock, and guarded by a boss of rock from any currents, which, when the Craig was submerged, may have flowed from the N.W. This deposit is of a red colour, and composed of sand and clay, derived probably from the Old Red Sandstone rocks situated in Arran and other islands to the north. In this deposit, Mr Macartney says he gathered a number of pebbles, striated or scratched, consisting of quartz, and metamorphosed slates and shales.

Mr Wünsch of Glasgow informed me that he had found granite pebbles on Arran.

(2.) At Ardrossan (Ayrshire), on the beach, there are numbers of conglomerate boulders, distinguishable by the prevalence of white quartz pebbles in the rock.

At Lamlash Bay, in Arran, I noticed boulders of a similar conglomerate.

Have they all come from some northern quarter?

(3.) At Millport (Buteshire) there are two large boulders of coarse grey granite, which are used in the harbour there as "*pauls*" for ropes from ships.

(4.) Near Beith (Ayrshire) there is a hill called *Cuffs*, which Mr Craig of Beith took me to visit. On the north side of this hill he pointed out many small-sized boulders of grey granite at a height of about 560 feet above the sea. The felspar crystals in it are

of a large size and very white colour, much resembling those found in the Arran boulders. Cuffs Hill consists of porphyry. It is surrounded by Carboniferous strata.

7. On a review of the facts stated in these notes regarding the Arran boulders, it seems probable that those described had been brought from the north, judging by the way in which they lie, and also by their composition.

With reference to the absence of boulders from Brodick Bay, and to their abounding along the coast both north and south of that bay, what occurred to me was, that if the boulders were brought from the north by floating ice, the rocky ridge running down from Goatfell peak (a mountain 2874 feet high) to the north point of Brodick might have had the effect of diverting the current in a S.E. direction, which would carry the ice beyond the bay. That bay is at the lower end of a valley which runs up among the highest hills; and if the theory of glacier from these hills be adopted, the bay should have been crowded with boulders, instead of being free from them.

Big Cumbrae.—I was guided to the north end of the island by the Rev. Mr Lytteil. There, on the farms of Figgatoch and Balloch Martin, I found several large boulders of mica schist lying on Old Red Sandstone rocks. The largest measured 12 × 6 × 3 feet. But it may have been larger, much of it being below the surface of the ground. The longer axis lay N.N.E., which was also the direction of the hollow or small valley in which it lay.

On the 70 feet terrace one of the schist boulders was about 5 feet square.

At the S.W. point of the island (viz., Kennery point), above half a mile to the west of Millport, I found several other schist boulders, on the old 12 feet sea-terrace.

Little Cumbrae.—The rocks of this island are entirely a brittle claystone trap. The rocks at the highest part (near an old tower), at a height of about 400 feet above the sea, are very distinctly smoothed and grooved. Most of the smoothed surfaces slope down towards and face N. by W.

The only part of the island on which striæ were found is at the east side, near a small ruined fortress. A hollow or trench occurs between the knoll on which that ruin stands and the main body of

the island. Fig. 6, plate XIX., shows the trench apparently scooped out in the rock by some heavy agent which has passed through, smoothing it on both sides and striating it on one side. The direction of the trench is N.E. by N. As it is only the east side which shows striation, the striating agent, if it came from the north, must have moved from a north-westerly point.

The striæ can be traced longitudinally for about 30 yards.

The figure shows a boulder, B, resting on an upper part of the trench, where there happens to be a sort of shelf where it has originally been lodged.

This is the "Split Boulder" first noticed and described by Mr Smith of Jordanhill. Before it broke into its two fragments, its size must have been 8 × 7 × 6 feet. Though the boulder is a claystone trap, viz., the same rock as that composing the main body of the island, I do not think it has rolled down to its present position, but agree with Mr Smith, that it is a true erratic, having been brought by ice which probably jammed in the trench as it was passing through.

The island has a number of Old Red Sandstone and also of conglomerate boulders on various parts of it, very similar in mineralogical character to the strata which are seen on the shore to the N.N.W. at Rothesay and Toward. One of these conglomerate boulders is of archæological interest. It bears the name of the Belstane, and is supposed to have been in former times connected with the Beltane fires. There are markings on the stone which have evidently been made for some special purpose. One of these boulders, about 5 feet square, rests on rock, and may have been used as a "Rocking Stone." The Rev. Mr Lytteil pointed out this stone to me.

East Lothian and Mid-Lothian.

1. For a notice of several boulders see paper by Convener in "Proceedings of Edinburgh Royal Society" (7th July 1877).

2. Extract from paper on the "Physiognomy of the Lothians," by R. J. Hay Cunningham, in "Trans. of Wernerian Society for 1838," vol. vii.

"In this district little extent of country can be passed without

numerous rolled masses of rock occurring, which are not found *in situ*, but only in distant localities.

"On the coasts of Linlithgow and Mid-Lothian, in the valleys of the Pentlands and on their acclivities, and on the flanks of the Lammermuirs and Moorfoot range, we easily detect rolled fragments of granite, syenite, porphyry, mica slate, gneiss, quartz-rock, and varieties of greywacke, which are met with only in the central districts of Scotland, while an examination of them shows that they decrease both in magnitude and frequency, as we advance southward; a fact indicating that the aqueous currents (for to such only can they be referred) diminished in intensity as they were removed from the central parts of the island."

Professor Nicol of Aberdeen (in the "London Geological Society's Journal" for 1848, vol. v. p. 23) refers to "one angular block of mica slate, near Habbie's How, on the Pentlands, weighing (according to a measurement I made) 6 or 8 tons. Farther west, I found another block, also angular, of the same sort, weighing about $\frac{3}{4}$ of a ton. When it is considered that these masses must have been carried upwards of 40 miles, floating ice seems to be the only agent to which their transport can be ascribed. Blocks of a smaller size are very common;—some are of kinds of rock *which I have never seen in Scotland.* On one hill, 1500 to 1600 feet high, I found these travelled stones particularly abundant, *and apparently increasing in number from below upwards.* In some places they appeared to form broad bands running nearly in straight lines from N.N.W. to S.S.E., and without any reference to the present declivity of the ground, except *becoming more numerous towards the summit of the ridge.* These blocks consisted chiefly of trap rocks, especially basalt; the hill on which they rested being a red felspar or claystone porphyry."

3. On 29th Oct. 1879, the island of Inchkeith was visited, under the guidance of Colonel Moggridge, R.E., superintending the erection of fortifications there. The rocks consist chiefly of basalt and porphyry intruded among the Coal-measures of Fife and Mid-Lothian. In various places the rocks are covered with beds of boulder-clay, gravel, and occasionally sand. The inspector of works (Mr Beck) mentioned that at the east end of the island, when removing a bed of shingle (about 60 feet above the sea), he

picked up two pebbles of red granite about the size of a hen's egg. Thinking it curious that granite should be found there, he laid the pebbles aside and kept them for some time, but they had since been mislaid.

Having been told that a number of large pebbles of various kinds were seen at the west end of the island, on the beach, I went there, and found numerous pebbles of granite (both red and grey), gneiss, quartz, and hard Silurian rocks.

On the highest part of the island (which is 182 feet above the sea), and on portions facing the N.W., the rocks have been well planed down to even surfaces by some agency from the west. But no striæ were observed.

4. A short time ago I went, on the invitation of Captain John Macnair of Edinburgh, to examine two boulders lying at the side of the Water of Leith, on the farm of Whelpside, near Kaims and Dalmahoy hills, about 9 miles S.W. of Edinburgh. One boulder was $13 \times 10 \times 6$ feet, and the other $10 \times 8 \times 5$ feet; but the depth of either could not be well ascertained, being deeply sunk. They were both of them a hard porphyry, containing minute crystals of a black mineral like hornblende, in a basin of white felspar. The longer axis of both was E. and W. They were covered with striæ, long and deep, running also E. and W., and indicating a movement over them from due west.

I ascended the Kaims hill, situated about a mile to the N.W. of the boulders, and found its west side swept bare, with numerous large fragments of the rock of the hill (a hard sandstone) strewed over its eastern slope.

On my way back to Edinburgh I examined the whinstone quarry of Ravelrig, and found, on the natural surface of the rock composing the hill there, numerous examples of ruts and scoopings, all indicating an agency which had passed over the hill from due west.

Kirkcudbrightshire.

Large rounded fragments of granites and syenites are abundantly scattered over the Stewartry, and so arranged as to indicate that they have been dispersed by a force proceeding from the N.W.—

(Robert J. Hay Cunningham, "Highland and Agricultural Society's Trans." vol. viii. p. 716.)

Peeblesshire.

Reference made "to the boulders of gneiss, granite, and mica slate, which belong to rocks unknown in the hills of that county, and several tons in weight." They "seem to require for their transport more powerful agents than mere currents of running water. We can scarcely conceive these possessed of sufficent velocity to convey masses of such a shape and size along a level plain, still less over the summit of hills 1500 or 1600 feet above the level of the sea, and across many winding valleys. The most probable means of conveyance, not only for these, but for many of the smaller fragments, seems to be masses of ice floating in an ancient sea, by which the highest summits of these hills were then submerged."—(Professor Nicol, "Highland and Agricultural Society's Trans." vol. viii. p. 197.)

Roxburghshire.

1. Near Castleton, many blocks of granite—both red and grey—lie on the greywacke and also the carboniferous rocks, which must have come from hills to the westward in Dumfriesshire or Kirkcudbrightshire, 30 to 60 miles distant, crossing the valley of the Esk.

2. On Ruberslaw, a hill of greenstone, about 200 feet below the top, I fell in some years ago with a large block of greywacke. It was lying on Old Red Sandstone strata. The nearest greywacke rock is situated to the westward about 3 miles. Between these rocks and the position of the boulder, there is low ground, at least 800 feet below the level of the boulder, which it must have crossed to reach its site.—("Edin. Roy. Soc. Trans." vol. xv. p. 454.)

3. Near the village of Nesbit, about 8 miles S.W. of Kelso, there is a boulder of small-grained greenstone 8 × 7 × 5 feet, identical in composition with the rock of Penielheugh, a hill on which stands the Waterloo pillar, a structure of about 120 feet in height. The rocks where the boulder lies consist of Old Red Sandstone;

and they are well covered by beds of gravel and sand. The boulder is on a knoll, near the top, but a little to the N.W. of it. The longer axis is in a direction S.W. and N.E. Penielheugh Hill is situated to the S.W. and distant about a mile from the boulder. The hill is 774 feet above the sea—the boulder 224 feet above the sea. The exposed rock of the hill on its west side reaches down to about 400 feet above the sea.

That the boulder has been brought to its present site from Penielheugh, is evident,—the composition of the rock being the same in both. The Old Red Sandstone rocks which prevail generally in the district, reach up to within about 100 feet of the top of Penielheugh, but only on the *east* side. These strata are entirely absent on the *west* side, suggesting, therefore, the probability that the west side of the hill has been denuded of them by some agency which has come against the hill from the westward. This inference is confirmed by the fact, that on the sides of the hill facing the west, the igneous rocks are all *bared*, and many of them *smoothed;* whilst on the sides facing the east, no igneous rocks are visible, being covered by sandstone strata, with drift materials over these.

These facts will be better understood by reference to plate XVII. fig. 6, where P represents Penielheugh Hill, B the boulder. The strata in dark colour is the Old Red Sandstone formation.

On looking from the top of Penielheugh westward, a wide valley is seen in that direction, the Eildon Hills on the north, and the Minto Hills on the south.

Through that valley some agency has undoubtedly come, impinging with great force on Penielheugh; but whether a local glacier or a sea-current with floating ice, there is nothing to show, though the extensive beds of gravel and sand which abound in this district, at no great distance from Nesbit, seem rather to favour the latter theory.

Selkirkshire.

On the top of Meigle Hill, about 2 miles from Galashiels, there is a boulder which I was requested to come and examine. It is of this shape, and its size is $6 \times 4\frac{1}{2} \times 3\frac{3}{4}$ feet.

Its longer axis lies N.W. and S.E., the sharp end pointing N.W. The person who invited me to visit the boulder, and guided me to it, told me that he had, by means of a lever, moved the boulder about 9 inches from its original natural position. The boulder is a hard grey Silurian rock, much harder than the rock of the hill, which is also Silurian.

The boulder, being well rounded, seems to have undergone much friction; and there are hollows and scoopings on several parts, such as frequently occur on rocks long subject to the eddying action of water. The boulder is about 58 yards east from the apex of the hill. It appeared to be lying on gravel or other drift materials, and about 12 feet below the apex of the hill. The hill reaches to a height of about 1430 feet above the sea. Many other boulders occur near the top of the hill, all of the same Silurian rock, well rounded, but none quite so large as the one above described. Meigle Hill stands by itself, *i.e.*, there are no other hills of equal altitude within some miles. There can be no doubt that all the above mentioned are "*erratics*," but from what quarter brought there is nothing to show. It would be difficult, however, to conceive any other medium of transport than floating ice.

Perth and Stirling Shires.

In looking through the Committee's previous Reports, I find reference made to a boulder near Doune, a *conglomerate*, weighing about 900 tons. A full account of this boulder, of the gravel beds on which it lies, and of its probable parent rock, is given in my little book called "Estuary of the Forth" (Edmonstone & Douglas, 1871), to which it may be allowable to refer (page 41). There are,

besides many other *conglomerate* boulders—as at the following places:—

On Landrick Estate, one weighing about 360 tons (p. 43).

At Keltie Bridge (a mile east of Callander), one weighing about 60 tons (p. 45).

On Gartincaber estate, one weighing about 16 tons (p. 43).

On north side of Teith, below Landrick Castle, one weighing about 13 tons (p. 44).

In the Burn of Cambus, two weighing about 13 and 24 tons respectively (p. 44).

In the district traversed by the hill road between Doune and Callander, there are multitudes of *conglomerate boulders* of smaller size (p. 44).

At Cornton brick-work (between Stirling and the Bridge of Allan) I saw a small *conglomerate boulder* found in the clay-bed there.

On the rocks adjoining Stirling Castle on the north, I observed small *conglomerate boulders*, besides some of gneiss and greywacke (p. 39). At Loch Coulter and Gillies Hill, places about 3 miles south from Stirling, and from 400 to 600 feet above the sea, I found several *conglomerate boulders*, besides some of mica slate and felspar porphyry, evidently all brought from the N.W.

On Plean estate (4 miles S.E. of Stirling), besides boulders of granite, gneiss, greywacke, and whinstone, there were some of *conglomerate* (p. 46).

At Glenbernie, near Torwood (5 miles S.S.E. of Stirling), I found a *conglomerate boulder* about 6 feet square (p. 48).

On Dunmore estate (about 9 miles S.E. of Stirling) there is the Carlin Stone, a *conglomerate boulder* weighing about 10 tons.

This list of *conglomerate* boulders may be considered interesting, as the position of the parent rock is known, viz., the band which traverses the country at Callendar, running from that point N.E. towards Brackland, and S.W. towards Aberfoyle and Loch Lomond.

Assuming that the boulders have all come from this band of conglomerate rock, they show a transport from the N.W. They also show that they cover a wide district of country towards the S.E., not a district forming a valley, in which a glacier might have moved,

but a district at various heights above the sea from 20 to 600 feet or more.

The boulders seem to increase in size and number the nearer they are to the parent rocks.

In the accounts given of these boulders it will be seen that those which are somewhat elongated in shape, have their longer axis lying N.W. and S.E., and that where striæ occur, either on boulder-clay or on rocks, these striæ lie in the same direction (pp. 46, 60, 61, 64).

The conglomerate boulders are chiefly referred to, because they are the most numerous, and the position of their parent rocks is best known. But the other boulders of the district—granites, silurians, and porphyries,—all yield confirmatory testimony, as will be seen from the positions of their parent rocks, and also their own position.

One other feature in this district may be mentioned, viz., the direction in which the gravel-beds have been by some means scoured out, leaving escars or kaims. Thus (1) at and near Bucklyvie (10 miles west of Stirling) there are three elongated knolls of gravel, sand, and boulders, lying in an E. and W. direction, reaching a height of from 60 to 70 feet above the adjoining district.

(2). On Blair-Drummond lands, there is a knoll composed chiefly of sandstone rock, but partially covered with gravel. It is known by the classical name of the Naidds' Knoll, given probably by Lord Kames, a former proprietor. It is in length 90 yards, and in extreme height about 50 feet, with a width of about 40 yards at its greatest width, which is near the east end. The direction of the longer axis of this knoll is W.N.W. and E.S.E. At the head of the valley towards the west, the lowest level is what is called the Pass of Bolat, and that point bears W.N.W. from the knoll. The rock of the knoll is a soft red sandstone, which could have been worn into its present shape by a current flowing through the pass in an easterly direction.

(3). About 2 miles south of Stirling, there is a gravel hill called Coxit. Its length is about ¾ of a mile, and its greatest width 300 yards. Its height is from 80 to 100 feet. Its longer axis runs about N.W. and S. E. A current flowing from the westward down the valley upon Stirling Castle rocks, might have had a branch diverted towards the S.E., and have scoured out the drift deposits, as it flowed near St Ninians and Sauchie, leaving Coxit Hill as a

remnant of the drift. Between Sauchie and Gillies Hills (chiefly whinstone), which are near Coxit, there is a narrow valley running in a direction N.W. and S.E., which would help to guide a current running in the direction supposed.

(4). The long escar of gravel passing through Callendar Park and Polmont, extending for about 2 miles, runs in an east and west direction, because there, any current would flow in a direction approximatively parallel with the axis of the valley of the Forth.

II. *PROFESSOR HEDDLE'S NOTES.*

Ayrshire.

1. In the *Valley of the Stinchar*, a boulder of fine-grained claystone, about a cubic yard in size, lies near the hamlet of Poundland.

It seemed to be in its mineralogical character identical with the rock of the hill of Glassal, situated to the N.E., and also with a rock on the shore to the west near Bennane Head.

2. About half a mile to N. of Colmonell, at a height above the sea of about 200 feet, a dolerite boulder occurs 27 × 23 × 12 feet. Its longer axis lies N. and S.

It lies on till, and the till covers the serpentine rock of the S. slopes of Belhannie Hill.

A small boulder, apparently a fragment of the larger, lies to the south.

About 600 yards E. by S. of this boulder, viz., up the valley, a spur of the same kind of rock projects out of the serpentine of the hill.

3. Lower down the valley there is another boulder of the same rock. It has been rent into four pieces, and the impression is suggested that it had been rent in consequence of falling from a height. It also rests on till. The fragments indicate the boulder before being broken to have been 21 × 21 × 10 feet in size. Its long axis is also N. and S.

4. On the shore, a little to the north of Lendalfoot, there lies an Old Red Sandstone conglomerate boulder, 8 × 6 × 6 feet. It is undis-

tinguishable from the conglomerate of Wemyss Bay, situated about 30 miles to the north.

Argyleshire.

Colonsay.—The rocks near the place in this island where the steam-boat calls, viz., on the N.E. side, were found to have been smoothed in a line bearing W.N.W. and E.S.E.; but from which direction the smoothing agents had come was not ascertained.

North Uist.—At Loch Maddy the rocks were found to have been smoothed every where, and in the same line as at Colonsay.

There are localities which show unmistakably that the smoothing agent had followed a course from west to east, or rather from the north of west. But the hollows or trenches between the higher grounds and the strike of the old gneiss strata have exercised some influence in diverting the smoothing agent, sometimes one way and sometimes another.

The two trap islets, Maddy More and Maddy Beg, porpoise-nosed to the west, and cliffy to the east, vouch for the direction of flow of the agent which conferred upon them their striking forms.

Whilst at Loch Maddy, I was accompanied by Mr Harvey Brown, who has written several well-known works on the natural history of Scotland, and has noted glaciation with an active eye, and an intelligent and independent mind.

Mr Brown had lately returned from a visit of some duration to Newton, on the coast of North Uist, where he had Mr James Thomson of Glasgow as a companion. He furnished me with a sketch and description of a boulder which lies on sloping ground to the S.E. of Newton. It is $13 \times 5 \times 4$ feet. It lies with its longer axis pointing N.N.W. and S.S.E. Another is $9 \times 5 \times 5$ feet. He stated that Mr Thomson and he had spent some time in examining the glaciation of the neighbouring shore, and found that all the rocks were glaciated from the N.W. He suggested my applying to Mr Thomson for farther information. Mr Thomson, in reply, stated that the glaciation on the west shore of the Long Island was all from the west, varying occasionally between N.W. and S.W., and he added an expression of surprise that any one could have made the mistake of not seeing this fact, it was so palpably evident.

Harris.—On Gilebhal Glass, the southern flanks are striated up

to a height of 500 feet, apparently by ice which came through the gorge of Tarbert from the west. Above this height the glacial striæ strike down the slopes of the hill in every direction.

On the S.E. slopes of the hill, there are portions of the ribbed and striated rock which have been torn up, and carried but a short distance, then let down and fractured in the fall.

Clisham and Langa have but few boulders; those on the south spur of Langa reach a height of 1400 feet, which is nearly the upper limit of the glaciation of these hills.

While the glaciation of the east and west trenches between the Harris hills shows a course of transit from west to east, the valleys of the *highest* hills showed ice to have passed down them from the higher level, whatever the direction of these valleys may be.

West Loch Tarbert.—Though rock does appear between the eastern and western arm of the sea which impinge here so closely upon one another as to warrant the above common appellation, yet the isthmus is for the most part made up of boulder-studded till. One or two of the boulders are of a close-grained hornblendic rock, and doubtless have been portions of a band of rock of an identical character situated a few hundred yards westward on the north shore of Loch Tarbert.

As the nature and structure of this crypto-crystalline bed is very marked and unmistakable, I regard the above as unimpeachable evidence of the course of the ice through the pass; and it must stand as such till a similar bed is found on the shores of East Loch Tarbert. For such I searched without success, though I found a characteristic bed of graphic granite, no fragment of which, however, did I find in the till which plugs the throat of the pass.

Glen Scramble.—This deep glen lies between Gilabhal Glass and Skiam Hill. At the bridge which crosses the stream issuing from the glen, I found a number of loose masses of an igneous rock, identical with a rock forming a dyke coming out above the bridge. These masses, therefore, have come down the glen, viz., from the east; but if they were brought down by ice, there could have been no great mass of ice, the distance of conveyance being quite trifling in amount.

Scalpa Island.—Walking eastward from the village, I fell in with a boulder, 7 × 6 × 6 feet, of a characteristic granite, butted up against the rocky steps of a small knoll of gneiss rocks on its east side, about 35 feet above H.W. mark. A sketch of this boulder is given

on plate XVIII. fig. 6. On the face of the hill directly opposite on the Harris shore, situated to the N.W., a great bed of the same granite rock is distinctly visible. Two other granite boulders similarly "stopped" occur to the east of this one.

Shiant Islands.—On the *upper* surface of these islands, three in number, all of basaltic trap, and reaching to a height of about 500 feet, there are no boulders of any *foreign* rock. On the southern island there is a line of boulders not much rounded, which lie directly east of a spot where there has been a palpable rending.

Two of the islands are connected by a ridge or "ayre" of loose materials, over which the waves now occasionally roll. On the western slope of this "ayre" there are much worn fragments of two foreign rocks, viz., hornblendic gneiss and Cambrian sandstone.

The gneiss blocks are about 2 cubic feet in size. The conglomerate blocks are sometimes as small as eggs; two of these, but none of the gneiss, were found on the east side of the "ayre."

On a stretch of shore along the N.W. side of the most northern island, conglomerate blocks also occur.

The only place in this part of Scotland where I know of a similar conglomerate rock is on the Eye Peninsula of Lewis, a short distance east of Stornoway, and about 30 miles to the north of the Shiants.

There is one other feature about the Shiant Islands which seems worthy of notice. The two highest islands, viz., *Garbh Eilan* (rough island), and *Eilan an Tighe*, lie north and south of one another; whilst the third, viz., *Eilan Mhuire*, lies to the east, and does not reach so high a level as the other two.

The upper surface of the two largest and highest islands, both when viewed from a distance and when examined in detail, present such soft and gently-sweeping risings and hollows that ice in some form or other appeared to have passed over and pressed on the surface of the rocks. It had evidently gone over *Eilan an Tighe* from W. to E., and over the southern part of *Garbh Eilan* (lying to the north) in the same direction, but over the higher parts and main bulk of the island from the S.W.

This movement and direction of the ice on these islands is corroborated by the position of a number of boulders on both of these islands consisting of the basaltic rock of the islands, which are all on the east side of the most southerly of these islands, and in the

more northerly to the east of one or more spots where there has been a palpable rending of rocks by some powerful agent moving on them from the westward.

Now, it is rather remarkable that the island of *Eilan Mhuire*, situated to the east of the other two islands, presents on its surface no traces of the same smoothing which occur on the other two islands. It is lower in level than the other two. If it was ice which passed over and rubbed on them moving towards the east, why did not it also pass over and rub on *Eilan Mhuire?* If it was a sheet of land ice, the fact of *Eilan Mhuire* being a little lower in level should rather have ensured contact by the ice. If, however, the ice was floating, it may have passed over the lower island without reaching it.

Skye.—An examination of the north-east part of the island from Aird Point to Portree was made, chiefly along the coast, and partially among the hills.

While there was found throughout evidence of vast denudation with frequent rounded contours (as along the line of cliffs above the Kilt rock), the rocks nowhere bore groovings or even scratchings.

The cols between the numerous heights of the central range of hills were narrowly examined, as, in the case of a movement across the island from either N.W. or N.E. these hills must have been subjected to a great amount of "scour."

At the several cols, averaging about 1300 feet above the sea, the water-sheds which fall to the south commence with the most singular precipitancy, there being hardly a yard or two between the brink of the precipice (which falls sheer to the N.E.) and the trickling of a marshy stream flowing in the opposite direction. Between many of these cols, peaks of rock shot up to a height of 2000 feet and more.

There were no hollows and no contours which could be assigned to ice. The slope on both sides of the stream-trench was such as would result merely from the sliding soak of water.

No true boulders were any where to be seen. That the summit of this range has not been ice-worn, may be deduced from the abruptness with which fragments of an upper bed of basaltic columns shoot up with a pillared steepness which show no rounding of their angles, or abrasion of any of their terminations.

A loose pillar (of which a sketch was taken) points the same way.

This pillar, retaining all its original sharpness of angle, lies on its side at the very highest part of the whole range.

Though there is no evidence that ice has been over the top of these cliffs, there is evidence that it has been at the *bottom*.

The southern shore of Stainchol Bay is, with the little island at its eastern horn, stretched like a half-opened hand, so as to catch everything which may have been carried from the north along the eastern shore.

Among the rounded masses lying on the beach, there are blocks of the same Cambrian conglomerate which occurs at the Shiant Islands, and of a larger size.

On account of the position of Stainchol Island, it is not likely that these could have come from any point east of north.

On the island itself, no boulders were seen except on the S.W. shore; several consisting of dolerite, in which labradorite is well seen, lie here. A rock of the same nature occurs about 50 yards to N.N.W.

Loch Torridon and Loch Maree.—The position of "*The thousand hills*" (consisting of dirt cones and delta heaps) in Glen Torridon, and the smoothed rocks at the head of the glen, leave no room for doubt that a true glacier had descended this glen from the north and east. But, on the other hand, the till at the very summit-level between Glen Docharty and the head of Loch Roisk, has indubitably been water-dressed, and the dressing agent seems to have come up Glen Docharty.

The ice had apparently come out of every corry of the eastern sides of Leagach and Eye, to merge into the Torridon glacier.

But, on the other hand, there were found on *Scuir na Convaran* (a N.E. quartzite spur of *Ben Eye*) boulders of hornblende rock, hornblendic gneiss, and of Cambrian sandstone.

The hornblendic boulders were very similar to the hornblende of *Ben Arrichar* on the north shore of Maree, 13 miles to the westward.

As they lay much in line, in order to ascertain that they were not merely the turned-over fragments of a vein, though such a thing was most improbable, the ground was carefully scanned by several pairs of eyes, but no fixed mass was found.

An opposing spur of *Miall Ghubhais*, called *Carn a liadh* (grey

cairns), which lies N.W. of this, was distinctly hummocked at a height of about 950 feet above the sea.

Black Mount district,—having Loch Levin on the north, Moor of Rannoch on the east, and the Linnhe Loch on the west.

1. A train of boulders having been noticed by me on the north slopes of the valley of the *Beathard,* west of Loch Tulla, viz., on the low slopes of *Stob Ghabhar* and *Ben Toaig,* and also several huge blocks upon the shores of *Loch Dochard,* I felt a desire to seek for the parent rocks.

The boulders on *Stob Ghabhar* were of a pecular white granite, and were in size on an average up to 10 × 10 × 7 feet.

Ben Toaig and *Ben Terrick* are hills of gneiss. In the col between these hills, at a height of 2530 feet, the same variety of white granite boulders were found, with an average size of about a cubic yard, much worn. There was glaciation on the rocks (but much effaced), from S.W. to N.E.

Stob Ghabhar is also a gneiss hill. No boulders were seen except on its southern slopes, *i.e.,* at the spot already mentioned.

Ben Starrav was ascended. Its rocks were different from that of the boulders, as they consisted of a flesh-coloured granite.

The hills called *Scon Ghearraen* and *Meal Odhar* were next examined, forming west spurs from *Stob Ghabhar.* The rocks on them, as well as on *Glass Bein Mohr,* were found to be granite, but not exactly the same as that of the boulders.

Albannach hill was found, from its first eastern cliff to its summit, to consist of granite *identical with that of the boulders.* Blocks of the rock strewed its cross-corries in numbers; and the whole process of boulder formation may be said to be still displayed upon its slopes.

On the east and south-east sides of the hill there seemed to have been ice moving towards the south and towards the east.

On its northern side, similar traces were visible in the great corry under the sharp peak, showing a movement first to the north, and then a confluence with glaciation from a west corry of *Meall Targuinn,* thereafter curving westward, and sweeping towards Glen Etive.

This great hill, reaching to a height of 3425 feet above the sea, seems to have been the cradle of local glaciers, and also the source from

which the boulders near Tulla had been carried about ten miles in a direction E.S.E.

As it was thought desirable to see whether these boulders could be traced farther to the eastward, I tracked them back to the west and north shores of the lake, and thereafter for 2 or 3 miles up the course of the water of Tulla. They evidently diminished in numbers towards the east. Some of the boulders at Loch Tulla were about 8 cubic feet in size.

A search was next made along the southern range of hills, of which Meal Buidh, Ben Creachan, and Ben Achallater are the highest. But no boulders of the same or of any kind were found on them.

These boulders, therefore, had been carried, as it were, in a stream, and one of no great width, towards the S.E.

The valley, which gradually ascends westward from Loch Tulla towards the great massive hill of Starrav, becomes very narrow immediately to the east of Loch Dochard.

If any powerful agent passed through this valley eastward, it is probable that there would be great obstruction and a violent pressure on and rending of the adjoining rocks.

The lower part of the pass contains much till, and occasionally rock rises up through the till with finely smoothed hunches, showing striations from the W.N.W.

On the south side of the lake there are some enormous boulders, mostly angular, several of which are broken or fractured, as if by falling from a height. A sketch is given of one of these, fig. 7 on plate XVIII., as it is the largest I have seen or heard of in Scotland, except one in Arran. Its size is $45 \times 22 \times 26$ feet, and amounting therefore in weight to about 1900 tons. It consists of mica gneiss, and lies upon till. The view in the figure is taken from N.N.W. Other boulders of a similar rock occur at the same place, nearly equally large.

The hill immediately to the south of this boulder is composed of a similar sort of rock; so that very possibly, nay probably, the boulder may have been detached from the hill. But it is so far from the hill, and the intervening ground is of such a nature, that nothing but ice could have brought it into its present position.

The rocks at this place are much rounded, and show striæ running

W.N.W. and E.S.E. The striating agent unquestionably here came from the westward.

2. *Loch Creran.*—On the east side of this loch there are a number of boulders, some of very large size, of which notice was taken in the Committee's two last Reports.

My attention was drawn to these by our Convener, so that in the event of my visiting that district during the past summer I might endeavour to discover from what quarter these boulders had come.

I was glad to find myself able to comply with this request, and I spent several days in examining the district in question.

On the banks of the Creran there are two distinct classes of boulders, differing in mineralogical composition.

Those in the lower part of Glen Creran, near the bridge at the head of the loch and between Invercreran House and Fasnacloich, are much weather-worn, dense in structure, and dark in colour. The hornblende in them is dark-brown in colour, with but little felspar, and they contain a little bronzy biotite.

In a higher part of the glen, at and above Fasnacloich House, the boulders have much felspar, which is pale in colour; also hornblende which is always green, sometimes light-green, and a little quartz, but almost no biotite.

The rocks of the glen adjoining the places where both sets of boulders lie are quite different from the rocks composing the boulders; I therefore made a diligent search among the hills in the neighbourhood for the parent rocks.

The first-mentioned set of boulders, which I may call the *Invercreran* boulders, I found as regards mineral composition to be the same, or very nearly the same, as a band of rock in the *Coire Dhu* of *Fraochaid*, at a height of from 1500 to 1700 feet above the sea. This corry leads up from Glen Creran about 4 or 5 miles to the N.N.E. of Invercreran. The only mineralogical difference which I could detect was, that in the rocks on the hill, there was perhaps rather less biotite.

The place where the rock composing the Fasnacloich boulders was found is in a col lying a little north of the fountain-tarn of the River Durer, a river running into the Linnhe Loch at Coil Bay. The col lies between *Stob Coire Dhu* and *Stob Coire Ruadh*, at a height

of 1940 feet. A number of blocks of this rock were found by me at the west foot of *Miall an Aodain,* a hill situated to the eastward.

How these two sets of boulders have been carried to their present positions is a question on which I have yet formed no decided opinion. As perhaps bearing on that question, however, it is right to mention that the rocks near the fountain-tarn of the River Durer, at a height of 1940 feet, are much glaciated and apparently from the west.

Striæ occur on a clay-slate rock about $\frac{1}{4}$ of a mile south from the place just mentioned, just before the ascent of *Stob Coire Ruadh* commences, and at a height of nearly 2000 feet, which show a movement from a little to the north of west. These facts seem to suggest that some powerful smoothing and striating agent had passed over this district from the west, and at a level exceeding 2000 feet above the sea. But west from the place where these smoothed and striated rocks occur, there are no hills so high as to produce a glacier, unless, indeed, a glacier had come through Glen Tarbert, which is a continuation of Loch Sunart, and crossed what is now the Linnhe Loch. Loch Sunart and Glen Tarbert occupy a hollow in the district which runs in a direction about W.N.W. and E.S.E.

It is, however, proper to add, that on the rock where these W.N.W. striæ occur, there are cross striæ overlying and cutting into these, which cross striæ indicate a movement from the S.W. These cross striæ being more sharp and minute than those first made, indicate more recent and also less powerful action. Can it have been that a sea existed at a level exceeding 2000 feet above the present level, with ice in it which was floating about in eddying currents, among what are now high peaked hills, tearing rocks out of the shallows, and pushing them over what were then submarine reefs?

In regard to the boulders at Ivercreran and Fasnacloich, they manifestly have come from the particular hills above specified; but whether dropped from floating ice, or carried by glaciers, it is with our present information impossible to say.

The striæ last mentioned, as occurring at the height of 2000 feet, pointing about W.N.W., bear on the top of *Fraochaidh,* a hill 2883 feet high.

But between that hill and the rock on which the striæ appear,

there is the deep gorge of the *Coire Ruadh,* which if it then existed would have conducted any glacier from that hill in a different direction, viz., towards the N.W., and not towards Loch Creran, which lies almost due south from *Fraochaidh.*

3. Upon the south slopes of *Stob Coire Ruadh* there is a boulder of the peculiar *porcelain porphyry* worked at Kentallen in Appin. The boulder is about a square yard in size. That it is a boulder, is evident from the fact of the rocks of the hill where it lies being totally different. Its height above the sea is 2250 feet. Now, a porcelain rock of exactly the same kind occurs among the *Ben a Bheithir* hills, at exactly the same height above the sea, about midway between *Craig Ghorm* and *Sgorr Dhonuill,* which is 3 or 4 miles to the N.N.W.

Assuming that the boulder came from that point, it must have crossed two valleys, each of which is less than 700 feet above the sea. How it could have crossed these, except on floating ice, it is difficult to see.

4. There is another boulder among these hills deserving notice. It is one of *Schistose Breccia,* lying on the east side of *Fraochaidh,* at a height of 2235 feet. The rock of the hill here is a *Schistose Gneiss.* Now rocks of *Schistose Breccia* occur between the two peaks of *Ben a Bheithir* just mentioned, situated to the N.N.W. This boulder in like manner must have been carried across the deep valley of the Durer to have reached its present position.

5. The col between *Creran* and *Allt na Gaorran* showed glaciation coming down from the corries of the rough *Sgorr na Ulaidh,* and out of a corry on *Ben Fhionnlaidh.* Many loose and angular blocks of the hills themselves, much confusion, and smashing of every kind, and the glaciated contours, twisting away to go down both glens in opposite directions, S.E. and S.W., is all that this locality discloses. The deep cut of *Glen Ure* showed evidence of movement down it.

In reviewing the information obtained by me regarding these Creran boulders, I feel that there ought to be farther study of them, before their mode of transport can be said to have been discovered. On the one hand, the clustered manner in which the boulders lie on the west of *Miall an Aodain,* and at two spots on the east side of Glen Creran, is suggestive of blocks having rolled over the terminal

front of glaciers; or perhaps of a lateral moraine, when regard is had to there being in some places a train of blocks in almost single file. But, on the other hand, I cannot shut my eyes to the possibility of these boulders having been carried or pushed into position by ice in another form, which came from the west through Glen Tarbert; and which, when it reached the Durer valley, was blocked by the huge masses of *Scuir na Ulaidh* and *Ben Fionnlaidh*, and then forced to sweep down the trench of *Glen Creran*, carrying boulders, and lodging them where they now lie.

District of Glencoe.—On the western grass clad slopes of *Sron Coire Odhar Beg*, a hill north of *Glen Coe*, in the higher part of the glen, a number of small boulders, much rounded, were observed of a peculiar granite. It was whiter and coarser grained than the well-known Ardshiel granite, and had a little hornblende in it.

They were in composition altogether different from the rocks of the hill on which they were first noticed, which consists of schistose breccia.

The hills to the eastward I had previously examined (*Ben a Chrulaiste* and others), and knew that they consisted of epidotic gneiss.

I therefore thought it probable that the birthplace of the boulders would be somewhere to the westward, so in that direction I proceeded.

On reaching the *Aonach-Eagach* range, I found the same boulders, fewer in numbers but markedly larger in size.

They were lying almost exclusively on the eastern side of the narrow ridge leading up to the summit, and almost on the summit of the nameless peak marked 2938 feet on the 1-inch Ordnance map. On the next rounded haunch (2880 feet) they were not seen; but they reappeared on the ridge as it ascended to the eastern peak of *Meall Dearg* (3090 feet), and almost up to the summit of the western peak (3118 feet).

Their position here was most peculiar. They lay upon a ridge not many times wider than their own bulk, and only on the eastern slopes of that ridge; while on the lower hills where they were first seen, the same boulders lay on the west slopes.

The parts between *Meall Dearg* and *Meall Garbh*, extending to about half a mile, are quite inaccessible, and could not be examined. But so far as the peaked rocks composing this district could be seen, no

boulders were on them, and, indeed, on account of their sharp-edged ridges boulders were not likely to have lodged on them.

On the hills of *Sgornan Fiannaidh* (3188 feet) and *Sgor an Caiche* (2430 feet), situated farther west, these boulders were not found, nor any rock of the same description.

I proceeded to the next hills, of somewhat greater height, about 6 or 7 miles to the west, to the south of Balachulish, viz., *Bhein Bahn*, *Sgorr Dherag*, *Sgor Dhonuill*, and *Creag Ghorm*.

In the bed of a stream which descends the steep eastern face of *Creag Ghorm*, at about 1500 feet above the sea, a belt of rock occurs identical with that of the boulders; also along a great part of the semicircular ridge which connects *Creag Ghorm* with *Sgorr Dhonuill*, at a height averaging 2250 feet, there is rock very similar to that of the boulders, there being rather less mica in it, and only occasional hornblendic crystals. *Biddian nam Bian* (3786 feet) was twice ascended, but it presented no trace of the rock sought for. But though the rocks at the two other places indicated were found to be almost identical in mineralogical composition with that of the boulders, I am not satisfied that they supplied the boulders. The spots where these rocks occur are only from 1500 to 2300 feet above the sea; whereas the boulders on some parts of the *Aonach Eagach* to the eastward were at a height of 3100 feet above the sea.

Therefore I admit that there must still remain some uncertainty as to the birthplace of these boulders. An attempt has been made by some geologists to explain how boulders may be transported to positions above the level of the parent rocks; and if that theory be correct it may overcome the difficulty referred to.

It is possible also that the rocks at *Creag Ghorm* and *Sgorr Dhonuill* may have formerly reached a higher level; and in that view it may be remarked that at present the rocks of these hills are even now, under the action of the weather, breaking off into huge blocks.

Of course it may still be possible to find the peculiar rock of these boulders on more elevated hills elsewhere. Ben Cruachan and other hills to the south and west reach a height of more than 3100 feet; but I have been on most of these hills, and I do not think that on any of them there are rocks which would produce the boulders.

It is therefore a fact of considerable importance bearing on any theory of transport, that these boulders on *Aonach Eagach* occupy positions much higher in level than any of the hills in a very wide extent of country; so that it is difficult, if not impossible, to adopt for them the explanation of any local glacier.

I have adverted to the peculiar position of the boulders on *Meall Dearg*, where at a height of 3100 feet they lay upon a ridge not many times wider than their own bulk, or rather on the sides of that ridge facing the E. or N.E. I am not able to offer any satisfactory explanation of this feature. I would like again to study the positions of these boulders. They must have been brought there by ice, which may have come from the N.W., and stuck there among the high peaks till it melted, and allowed the boulders to subside on or near the top of the ridge. My explorations about Glen Creran led to the supposition of a flow of ice through Glen Tarbert on the N.W. side of Linnhe Loch. This might possibly also account for the boulders on *Aonach Eagach*. But in that case, where could the parent rocks be?

(Though it does not seem to have any direct bearing upon the question, yet it may be well to record the fact that the bed of the Cona is, for a short distance, about midway between the little lake and the hamlet of Clachach, cut through a rock very similar to, if not identical with, that of the boulders.)

III. *NOTES BY WILLIAM JOLLY*, Esq.

On the Carried Boulders on the South Shores of the Moray Firth.

In answer to your request, I send some notes, supplementary to those of last year, on the above subject.

The Dirriemore Granite seems to be more widely distributed towards the east than I anticipated. Since last year, I visited the place where I had formerly found it *in situ*, on the road between Dingwall and Ullapool, where it appears in the valley of the Blackwater, about and below its junction with Strathvaich. *None of it*

has been carried *westwards* between this part of the valley and Loch Broom, a tract which I have examined more than once. It has been carried altogether *towards the east,* in accordance with the general slope of the country. This granite would seem, however, to occupy a wider and more elevated area in the Ben Wyvis mountains than is shown in the Blackwater, from which it has been borne and dropped along the south shores of the Moray Firth, after being carried down the several valleys that drain this range into the Cromarty Firth, as well as down through Strathpeffer, and down the lower valley of the Conon below its junction with the Blackwater near Tor Achilty, in Contin.

In the valley of the Alness, for example, it is widely distributed, having evidently come from some centre near its head waters. Good specimens of it may be seen round the village of Alness, and along the shore between it and Invergordon, skirted by the public highway. It has been carried across the Cromarty Firth, and scattered abundantly in large and striking masses *over the whole of the Black Isle,* from end to end. Good examples of it may be seen at its northern extremity round Cromarty, and along its central ridge on the road between that town and Fortrose, large pieces being easily seen on the moor near Peddieston, a few miles south of Cromarty, and along the road between Invergordon Ferry and the Sutors. It is also found extensively along the whole of the east shore of the Black Isle, and has been carried thence eastwards towards Buckie. It exists plentifully all over the Laigh of Moray, and may be well seen along the seashore there, especially between Burghead and Lossiemouth.

The Stratherrick Liver-coloured Conglomerate I have found numerous additional examples of, from its source on the east shore of Loch Ness north of Inverfarigaig, onwards to Lossiemouth.

There would seem, however, to be two varieties of conglomerate distributed throughout the Laigh of Moray—the above easily distinguished rock, and another consisting of more angular components and entirely without the liver-coloured quartzite or porphyry. Examples of the latter may be seen in the old quarry of Oolitic limestone at the classical Linksfield, near Elgin, embedded in the boulder-clay there, one of the masses on the south side of the quarry being very large. The Douping Stone on the top of the Califer Hill, east of Forres,

mentioned in my notes of last year, is certainly of the Stratherrick liver-coloured variety; but the block on the top of Roseisle Hill, also mentioned by me, may be of the other. This second conglomerate would seem, from various indications, to have been transported at an earlier period than the Stratherrick, for it is found embedded at greater or less depths in the prevalent boulder-clay of Morayshire; whereas the Stratherrick rock is seldom, if ever, thus buried, being confined more to the upper surface of the country. The glaciation of Morayshire shows two main directions of the scratches, indicating two lines of ice movement from the westward, as exhibited admirably on the ridge of Carden Moor, near Alves station. These scratches point respectively 13° N. of W., and 6° S. of W., as the directions from which the ice has come. The second conglomerate may have been carried across the Moray Firth from Ross-shire, like the Dirriemore granite, in the line of the former scratches.

The red orthoclase Kinsteary Granite, found *in situ* near Nairn, is very abundantly distributed from this point towards the east, onwards beyond Buckie.

Mr Linn of the Geological Survey, at present engaged in mapping the district round Elgin, has found these three rocks widely spread all over the Laigh of Moray, and has taken the fullest notes of the composition and positions of the various carried blocks there, which will be embodied in his map of the region, and will form an important contribution to the question of the transportation of rocks along the south shores of the Moray Firth.

I append some notes supplied to me by Mr Wallace of the High School, Inverness, mentioned in last year's notes, regarding their distribution on the north coast of Banffshire. These carry the account of the transport of boulders eastwards to Cullen. It would be most desirable that the Committee should, if possible, obtain information regarding their farther distribution through Aberdeenshire, and thus complete their story to the German Ocean.

IV. *NOTES BY THOMAS D. WALLACE, Esq.*

On the Carried Boulders in the Parishes of Enzie and Rathven. Banffshire.

Having revisited this district at Christmas 1879, and examined it more carefully than on former occasions, I found further proof of the eastern flow of the great ice-sheet that at one time traversed the whole of the southern shore of the Moray Firth. In the neighbourhood of the Enzie post-office, I found numerous boulders of the Dirriemore granite, none of them so large as those that were dug out during the excavations for the Buckie Harbour, and mentioned last year in the Committee's Report.

Numerous small boulders of the Elgin Cornstones lie scattered all over the lower part of the district. Several are to be seen in the Gollachy Burn, a little to the west of Buckie.

Conglomerate boulders are rather rare. Except the few remaining stones forming the "Stone Circle of Dryburn," near Portgordon, I found only one, about a quarter of a mile east from Dryburn.

A very characteristic specimen of Kinsteary Granite is seen close beside the harbour of Buckie. Smaller pieces may easily be picked up on the fields along the shore. A well-marked feature of the schists which underlie the Old Red Sandstone in this district, is the frequent occurrence of large veins of calc spar, quartz, and quartzites. Specimens of these are also numerous in the drift.

A fine specimen of Cairngorm (water-worn) was picked up by a labourer on the high ridge to the south of the district, locally known as the "Hill of Altmore." It measures 2 inches thick at the one end and 3 inches at the other. It is about 4½ inches in breadth. This man, ignorant of its value, took it to Aberdeen and had it polished on both sides by some friend at the granite works. This has rendered it quite transparent, so that one can read with the greatest ease anything placed under it.

One section of Boulder-Clay is deserving of notice. It is in the wood of Pathhead, on the estate of Cairnfield, a little to the south of the Enzie post-office. It consists of a fine plastic clay of a dark bluish-black colour, overlaid by the well-known red boulder-clay. The blue clay represents the denudation of the schists, and the red that of the Old Red Sandstone. Notwithstanding a very minute

examination of every burn in the district, I failed to find any of the blue clay on the lower ground. This I take to be an additional proof of the easterly flow of the ice.

As far as the boulder evidence in this district goes, it proves conclusively that the ice-flow was from the W., or a little to the S. of W.

All along the south shore of the Moray Firth there are scattered boulders of conglomerate, hornblende, and dirriemore (besides other) granites. In the neighbourhood of Inverness, these would indicate a drift from the N.N.W. and one from the S.W., both tending E., or a little to the N. of E. The boulders of hornblende might have come from the N.W. The only place where I have seen hornblende *in situ* in the neighbourhood of Inverness, near which are found numerous boulders of that rock, is at Raven's-Rock near Strathpeffer.

(Signed) DAVID MILNE HOME, *Convener.*

Mr Milne Home, Chairman of the Boulder Committee, after presenting the preceding Report, made the following remarks:—

I may explain that, to save the time of the meeting, and also to afford to members information regarding the operations of the Committee during the past year, copies of the Report were circulated with the billets for the present meeting;—and for the same purpose, as Convener of the Committee, I now proceed to give an abstract of the chief features of the Report.

I. *Boulders in Nairn, Moray, and Banffshire.*

I begin by alluding to the boulders in these counties, because the notes applicable to them are in some sense a continuation of the part of last year's Report applicable to these counties.

In these counties there are two classes of important boulders,—*Granites* and *Conglomerates.*

Of granites there are four kinds, distinguishable by the ingredients, and by the different districts where their parent rocks are situated.

There are, *First*, the boulders, consisting of a very peculiar granite, with lenticular pieces of dark mica, arranged in pretty regular layers, through a pinkish mass, giving to it some resemblance to a stratified deposit. The granite of these boulders has been identified by Mr

Jolly of Inverness with the granite rocks of the Dirrie Muir, a tract in Ross-shire situated to the west of Ben Wyvis, and lying about half-way between the east and west coasts. These boulders have been transported in a E.S.E. direction across the Cromarty Firth, over the district of the Black Isle, and across the Moray Firth, into the low grounds of Moray and Banff. The distance travelled must be nearly 100 miles. *Second*, there are two other granites, one red and the other grey, which have been transported from the hills forming the sides of the great Caledonian Valley,—called the Loch Ness granite and the Stratherrick granite.

These boulders also are found in Moray- and Banff-shires, and show a line of transport not quite the same as the Dirrie Muir granite, viz., about E. by N.

Mr Jolly says, that the Stratherrick granite boulders have been seen by him on the hills south of the Great Valley, up to a height of 1500 feet. But the Boulder Committee, three or four years ago, received through Captain White of the Ordnance Survey, notice of these boulders, having been found by his surveyors at heights of 2250 feet, the parent rocks being on hills 2900 feet in height. This fact is mentioned in the Committee's second annual Report.

These boulders, before reaching Morayshire, must have travelled also about 100 miles.

A fourth class of granite boulders in Morayshire and Banffshire is a beautifully pink-coloured rock, quarried at a place called Kinsteary in Nairnshire. No boulders of this peculiar granite are seen *east* of the parent rock.

What has now been said of *Granite* boulders, as regards transport, applies to the *boulders of Conglomerate*. There are two kinds of conglomerate rock forming them, and they come from different districts,—one in the Great Valley itself, which it crosses near the hill called Meal Fourvounie; the other in Ross-shire, at some distance to the north of the Great Valley.

The Committee have, in regard to these Moray- and Banff-shire boulders, obtained valuable notes from Mr Jolly and Mr Wallace, both resident in Inverness. The information given as to the position of the parent rocks is gratifying in this respect, that when, three years ago, many of these boulders were examined by myself, I drew an inference regarding the quarter from which they

had probably come, founded solely on the position and attitude of the boulders themselves, the correctness of which inference has now been confirmed by the discovery of the particular districts where the parent rocks are situated.

II. *Professor Forster Heddle's Explorations.*

The Professor's survey last year began on the West of Scotland, and extended from Ayrshire to Loch Torridon in Argyleshire; and also into the interior, near the districts called the Black Mount and Glencoe.

I was especially glad, on receiving the Professor's notes, to find that he had visited several of the islands of the Hebrides; because, as was explained in our last year's Report, the problem of the mode of transport becomes less complex on islands where there are neither hills nor valleys suitable for the formation of local glaciers.

1. The first island visited was *Colonsay*,—on which, however, nothing seems to have been found, beyond rock striations running W.N.W. and E.S.E., but which way the movement was, did not appear.

2. The next island was *Uist*. There, in like manner, the rock striations were W.N.W. and E.S.E., and it was there seen that the striating agent come from the westward. The Professor adds, that "the hollows or trenches between the higher grounds and the strike of the old gneiss strata have exercised some influence, in diverting the smoothing agent, sometimes one way and sometimes another."

At Loch Maddy in Uist, Professor Heddle met with Mr Harvey Brown, who had been for some time surveying there for objects of natural history, in company with Mr James Thomson, a member of the Glasgow Geological Society. Both of these gentlemen had also been studying the phenomena of boulders and striated rocks in the north part of Uist. Mr Brown supplied Professor Heddle with a note of the size of several boulders (which are specified in this Report), and he recommended the Professor to write to Mr Thompson for farther information. The Professor did so, and the answer he received from Mr Thompson was, "That the glaciation on the west shore of the Long Island was all from the west, varying occasionally between N.W. and S.W.;" and he "added (the Professor says) an expres-

sion of surprise, that any one could have made the mistake of not seeing this fact, it was so palpably evident."

3. In *Harris*, Professor Heddle found that on the southern flanks of the hill called *Gilebhall Glass*, rocks were striated up to a height of 500 feet, apparently by ice which (he says) came through the gorge of Tarbert from the west.

At this gorge, he found in the Till, boulders of a close-grained hornblende rock, doubtless (as he says) portions of a rock of identical character situated a few hundred yards to the westward.

The Professor adds, that as this crystalline rock is very marked and unmistakable, he regarded it as unimpeachable evidence of the course of the ice through the gorge.

4. *Scalpa* was next visited,—an island half a mile or so off the east coast of Harris. Here a granite boulder was "found *butted up* on its east side against the rocky steps of a knoll of gneiss rocks." A sketch of this boulder is given in the Report. With reference to the direction of transport, Professor Heddle mentions that the same rock of which the boulder is composed forms a bed in a high cliff on the mainland of Harris to the N.W.

5. *The Shiant* islands, which were next examined, are three in number. They also are off the east coast of Harris, about twenty miles to the N.E. of Tarbert.

On the surface of these islands, which are of basalt, no foreign erratics of any size were found. But on the shore of two of these islands, he found blocks of conglomerate and Cambrian sandstone, which had probably come from near Stornoway, about 30 miles to the north, where these rocks are *in situ*.

The Professor saw in the two largest islands, which lie N. and S. of each other, that some agent—it might be ice—had passed over them from the west, smoothing them, and pushing fragments of the trap rock towards the east. But he observed particularly that there was no such smoothing on the third island lying to the east; and the only explanation of this fact which occurred to him was, that the third island, being much lower in level than the other two, the ice may have passed over, without touching it—an explanation suggesting the agency of floating ice.

6. The next island visited was *Skye*, but the Professor was able only to examine the N. and N.E. portions, viz., from Aird Point to Portree.

He was surprised to find no boulders either on the coast or on the hills adjoining the coast, except on the small islet of *Stainchol*, at the mouth of Loch Staffin. On the shore of this loch there were blocks of Cambrian sandstone, a rock of which he had found pebbles on the shore of the Shiant Islands. On Stainchol, he also found a boulder of dolorite, containing much labradorite;—a rock of the same nature, was *in situ* about 50 yards to the N.N.W.

Professor Heddle further attests, that the rocks on the hills examined by him, which he ascended to above 1500 feet, "nowhere bore groovings or even scratchings;" and he states that "the *cols* between the numerous heights were narrowly examined by him."

Now these facts seem to have an important bearing on the question of boulder transport. Mr James Geikie, in his "Great Ice Age," p. 77, says, that "most of the islands which lie off the coasts of Scotland plainly indicate, by striations and other glacial markings, that ice has swept over them." He adds that "The most striking example of this is furnished by Lewis, the northern portion of the Long Island, which (says he) I found to be glaciated across its whole breadth *from* S.E. to N.W. The land-ice that swept over this tract, must have come from the *mountains of Ross-shire*—a distance of not less than 30 miles. Leaving the mainland, it must have filled up the whole of the North Minch (60 fathoms in depth), and overflowed Lewis to a height of 1300 feet at least."

This statement, made in 1877, was repeated in two elaborate papers read before the London Geological Society in 1878, in which it was maintained, "that the whole of the Long Island, from the Butt of Lewis to Barra Head, has been overflowed from the Minch by ice that moved outwards from the inner islands and the mainland."

Now this theory seems entirely at variance with the facts ascertained by Professor Heddle last year, and by myself in the previous year. If a mass of ice came from the Ross-shire hills, so great as to fill the Minch, overflow the Long Island to the height of 1300 feet, and to stretch from the Butt of Lewis to Barra Head, a distance of about 80 miles, it must have impinged on the island of *Skye*, and especially on the north-east part of it. But there, according to Professor Heddle, no boulders are to be seen, and even no groovings or striations of the rocks.

On the other hand, boulders and striated rocks, which in the

Long Island are plentiful, all indicate a movement from the N.W. —a direction the very opposite of that requisite for a great glacier from Ross-shire.

Mr Geikie, in a footnote (page 60), says that "Mr Campbell of Islay considered that the Hebrides Islands had been glaciated by sea-ice coming from the N.W.;—while, on the other hand, my observations in Lewis compelled me to believe, that the glaciating agent was land-ice streaming outwards from the mainland. My colleague, Mr Etheridge, Jun., who accompanied me during my last visit to the Long Island, also concluded, that the glaciations had been effected by land-ice coming from the S.E."

Whilst much weight is proper to be given to the observations and opinions of such experienced geologists as Mr James Geikie and Mr Etheridge, on the other hand it is only right to keep in view that the late Robert Chambers and Dr Bryce, though they wrote no papers on the subject, are known to have concurred with Mr Campbell; and I have reason to believe, that Mr Jolly of Inverness is of the same opinion.

7. *Black Mount District.*

(1.) Some white granite boulders, which were noticed by Professor Heddle on the shores of Loch Tulla, were, after a minute and laborious search, traced by him to a hill called *Albannach*, situated about 10 miles to the W.N.W. Professor Heddle having ascertained the line of transport, next tried to find whether the boulders covered a large space transversely. The result of this search was to show that (to use the Professor's words) "these boulders had been carried, as it were, in a stream, and one of no great width, towards the S.E."

(2.) Notice was next taken of several large boulders, one weighing no less than 1900 tons, in a valley, which becomes very narrow to the east of Loch Dochard. The Professor says, "If any powerful agent passed through this valley, there would be great obstruction and a violent pressure on and rending of the adjoining rocks. The lower part of the pass (he says) contains much till; and occasionally rock rises up through the till with finely-smoothed hunches, showing striations from the W.N.W." In reference to the large boulder above referred to, Professor Heddle gives his opinion that "nothing but ice could have brought it into its present position."

(3.) The *Glen Creran* boulders having been referred to in the two last Reports of the Committee, with a confession of uncertainty as to the source from which they had come, Professor Heddle, in compliance with my request, kindly undertook a renewed survey of the district.

The result has been that the Professor has found rocks on the hills 4 or 5 miles from Glen Creran to the N.N.E. identical in composition with the boulders. But how the boulders were carried from these hills, where the parent rocks are from 1700 to 2000 feet above the sea, to Glen Creran, which is only about 200 feet above the sea —*i.e.*, "whether dropped from floating ice, or carried by glaciers," "it is (observes the Professor), with our present information, impossible to say."

He found on the hills containing the rocks of the boulders numerous *striæ*, which showed "that some powerful smoothing and striating agent had passed over this district from the west, and at a level exceeding 2000 feet above the sea. But west from the place where these smoothed and striated rocks occur, there are no hills so high as to produce a glacier, unless, indeed, a glacier had come through Glen Tarbert, which is a continuation of Loch Sunart, and crossed what is now the Linnhe Loch. Loch Sunart and Glen Tarbert occupy a hollow in the district which runs in a direction about W.N.W. and E.S.E.

"It is, however (he says), proper to add that on the rock where these W.N.W. striæ occur, there are cross striæ overlying and cutting into these, indicating another and more recent agency from the S.W. These cross striæ being more sharp and minute than the first, indicate more recent and less powerful action. Can it have been that a sea existed exceeding 2000 feet above the present level with ice in it, which was floating about in eddying currents among what are now high-peaked hills, tearing rocks out of the shallows, and pushing them over what were then submarine cliffs?"

(4.) In this part of his notes applicable to the Glen Creran district, Professor Heddle refers to what he calls "*a boulder of the peculiar porcelain porphyry* worked at Kentallen in Appin." That it is a boulder is evident from the fact of the rocks of the hill where it lies, being totally different. Its height above the sea is 2250 feet. Now, porcelain rock of the same kind occurs among the *Ben a*

Bheither hills at exactly the same height above the sea, about midway between two other hills whose names are given, 3 or 4 miles to the N.N.W.

Assuming that the boulder came from that point, it must have crossed two valleys, each of which is less than 700 feet above the sea. How it could have crossed these, except on floating ice, it is difficult to see.

(5.) There is another boulder in the same district of *Schistose Breccia* at a height of 2235 feet. The parent rock was found at some distance to the N.N.W. "This boulder (the Professor says) in like manner must have been carried across the deep valley of the Durer to have reached its present position."

(6.) A very interesting account is given of boulders in the neighbourhood of *Glencoe.* Being much rounded, they suggest a long transport, and were "of a peculiar granite," somewhat like "the well-known Ardshiel granite," only "whiter and coarser grained."

Being "altogether different from the rocks of the hill on which they were first noticed, consisting of a 'schistose breccia,' the Professor resolved to seek for the parent rock."

Thinking, from his knowledge of the rocks to the *eastward,* that they were not likely to have come from that quarter, he set out on a hunt in a westerly direction. On reaching the *Aonach-Eagach* range of hills, he recognised the same boulders on them, "fewer in number, but markedly larger in size."

He followed them up to the first summit of the hill, which was 2938 feet; and, proceeding still further west to a hill called *Meall Dearg,* he found the same boulders first at 3090 feet and eventually "almost up to the summit of the western peak at 3118 feet."

The Professor says that "their position here was most peculiar,—they lay upon a ridge not many times wider than their own bulk, and only on the eastern slopes of that ridge."

Proceeding still farther west to other hills (which are named in his notes) at from 2400 to 3200 feet, the Professor did not find either boulders, "or rock, of the same description;" but on proceeding to the next hills, of somewhat greater height, about 6 or 7 miles to the west, he found at two spots, the kind of rock he was in quest of. He however adds, that "though the rocks at these two spots were almost identical in mineral composition with that of the

boulders, I am not satisfied that they supplied the boulders,—for the spots where those rocks occur, are only from 1500 to 2300 feet above the sea,—whereas the boulders on some parts of the *Aonach-Eagach*, to the eastward, were at a height of 3100 feet above the sea.

"An attempt has been made by some geologists to explain, how boulders may be transported to positions above the level of the parent rocks; and, if that theory be correct, it may help to overcome this difficulty."

"But it is a fact of considerable importance, bearing on any theory of transport, that these boulders on *Aonach-Eagach*, occupy *positions much higher in level than any of the hills in a very wide extent of country*, so that it is hardly possible to adopt for them the explanation of any local glacier."

"I have adverted" (says the Professor) "to the peculiar position of these boulders on *Meall Dearg*, where, at a height of 3100 feet, they lay upon a ridge not many times wider than their own bulk, or rather on the sides of that ridge facing the E. or N.E. I am not able at present to offer any explanations of this feature. I would like again to study the position of these boulders. They *must have been brought by ice*, which may have come from the N.W. and stuck there among the high peaks, till it melted and allowed the boulders to subside on or near the top of the ridge. My explorations about Glen Creran, led to the supposition of a flow of ice through Glen Tarbert on the N.W. side of the Linnhe Loch. This might also possibly account for the boulders on *Aonach-Eagach;* but, in that case, where could the parent rocks be?"

This query by the Professor, Where could the parent rocks of these boulders be? he leaves unanswered; and, no doubt, it is a query more easily asked than answered. It would, therefore, be presumption in me even to suggest an answer. But the query reminds me that, two years ago, I sent specimens of the Loch Creran boulders to Professor Judd of London, an eminent geologist well acquainted with the rocks of the West Highlands, to ask him, whether he knew of rocks anywhere like those of the boulders, and he gave a decided opinion that rocks of exactly the same kind existed in Mull and Ardnamurchan. Now, these places are to the west of the boulders referred to by Professor Heddle, and

it is from the west that he thinks they came. Moreover, in Mull, the hill of Benmore is 3180 feet above the sea, whilst in Ardnamurchan there are hills nearly that height. It strikes me, therefore, that it would be very desirable, if Professor Heddle could, in the course of this summer, visit Mull and Ardnamurchan to see whether he agrees with Professor Judd's surmise on this subject.

III. *Convener's Notes.*

The points brought out in these, are very unimportant, compared with those of Professor Heddle and Messrs Jolly and Wallace of Inverness.

1. The boulders in *Cantyre* I found had, on the south and east coast, come apparently from some point due north; those on the west coast, from points varying between N.W. and N.N.W.

2. In *Arran*, the boulders on the east coast, which were all that I examined, seem to have moved in a direction from about due north.

3. In the *Cumbrae* islands, they seemed also to have come from due north.

4. In *Loch Long* and the *Gairloch*, the boulders showed transport from points varying between N.N.W. and N. by E., which happens also to be about the axial line of the valleys in which the boulders lie.

5. In the hills to the north of *Loch Fyne*, I was rather surprised to see the smoothed rocks facing N. and N.E., and the boulders lying with their longer axis in much the same direction.

6. When I reached *Loch Awe*, I found the boulders among the hills, at from 900 to 1000 feet above the sea, indicating in like manner transport from the N.N.E.

This deviation, at several places in the interior of the country, from the N.W. direction which is so prevalent elsewhere, at first rather surprised me; but it probably does not on principle differ materially from the fact, that occasionally on the same rock, or on the same boulder, there are separate sets of striæ. If these striæ are produced by currents which run first in one direction and thereafter in another, a similar explanation might apply to the variations of direction over a large district of country.

For example, Professor Heddle, as we have seen, takes notice of

such variation in *Uist*, and on a larger scale among the hills at Glen Creran; and the boulders in Nairn and Morayshire have evidently been brought by currents which came from different points.

If in the North of Scotland, the normal direction of the current was to the S.E., it is probable that the deep trench of the Great Caledonian Valley running about E. by N, with a range of hills on each side 2000 feet high, would there cause a deviation in the direction of the current. As the sea subsided from one level to another, the currents would change in directions.

Examples were seen by me last year in the Lewis, of a change even on the same hill. At the top, the direction was as usual N.W., near the bottom, it was from due W. or W.S.W.

Among the hills south of Loch Awe, I found a large boulder perched on a peak of rock in a remarkably precarious position. It is shown on the diagram. By a glacier it certainly could not have been brought, there being neither hills nor valleys to form a glacier. If it came by floating ice, the ice might be arrested by the peak, and when it melted, the block which the ice carried, might remain.

7. The largest boulder which I have yet seen, was found by me on the west coast of Argyle, in Loch Killasport. Calculating by its cubical contents, it weighed about 2770 tons. This boulder, and many others of large size, were on the sea shore, and half a mile at least from any sea cliff, old or recent. I felt convinced from their situation, and also from the direction of their longer axis, that they had all come across the sea from the N.W.

8. A short time ago, my attention was called to a boulder, $9 \times 8 \times 6$ feet, in Roxburghshire, weighing about 16 tons. On examining it, I found that it was of exactly the same rock as that which composes the Penielheugh, the hill on which the Waterloo Pillar stands. It is about a mile to the east of the hill, and has evidently been floated to its present position by ice. The hill also presents other facts of no small interest bearing on the transport of boulders. The west side of the hill has been swept bare, so that the trap rocks stand out like the bones of a skeleton with the skin and flesh off, whilst the east side of the hill is covered by soft Old Red Sandstone, as well as by sand and gravel. This place affords undoubted evidence of sea with floating ice, which stripped the hill and carried fragments to the eastward.

Whilst the view I take in regard to the transport of boulders, and the striation of rock surfaces in Scotland is, that these phenomena were in most instances due to ice in a sea, which reached to our highest mountain tops, I admit that there are traces also of land ice in the form of local glaciers. In last year's Report I pointed out what appeared to me clear evidence of glacier action in Glencoe; and Professor Heddle also recognised glacier action on the west coast near Loch Torridon. But my idea is, that these glaciers must be referred to a period antecedent to the submergence of the land, for we find those traces of glaciers in many places covered over by thick beds of gravel, sand, and clay which could only have been deposited by the sea.

DAVID MILNE HOME, *Convener*.

On 21st May 1880, at a meeting of the Council of the Society, the Committee was reappointed, with the addition of General Bayley and Professor Duns, D.D.

5. On Two Masks and a Skull from Islands near New Guinea. By Professor Turner.

These specimens have recently been presented to the Anatomical Museum of the University, by J. Wharton Cox, Esq., who had received them from his father, Dr Cox of Sydney, the well-known Australian naturalist.

The masks had been procured by Dr Cox from missionaries, and were either from the island of New Ireland or New Britain, in proximity to the north coast of New Guinea. They were both formed of the frontal and facial bones, on which a face had been modelled in a composition, formed of a mixture of a resinous substance with earth or clay. This artificial face had then been painted with red, black, and white pigments. The larger mask was hollowed out behind, by the removal of the sphenoid and ethmoid bones, so that it could be adapted to the face of a wearer, and a bar of wood was fastened transversely across the hollow, which the wearer had evidently used for holding the mask between his teeth; as the mask had both the eyelids and lips

separated from each other, the wearer could both see and breathe through these openings.

The smaller mask was not capable of being closely adapted to the face of a wearer, for the sphenoid and ethmoid bones were in position, and the orbits were filled up with a composition similar to that employed in modelling the face; and an artificial eye formed of the operculum of the shell of a mollusk was fixed in each orbit. An artificial tongue formed of a piece of bright red cloth folded on a wooden framework partially projected through the open mouth. Whiskers and beard, which in the larger mask were modelled in the hard composition, were in the smaller mask formed of vegetable fibre, and from their mode of arrangement gave a pantaloon-like character to the face. This mask may have been used as an ornament, or as an object of worship.

Warrior Island, from which the skull was procured, is an island off the south coast of New Guinea, and to the north of Torres Straits. The skull was smeared on the forehead and face with streaks of red pigment. The orbits were filled up with a hardened material, to which lozenge-shaped pieces of mother-of-pearl to simulate eyes were attached. A plug of wood $3\frac{1}{4}$ inches long, cut so as to represent an artificial nose, was inserted into the anterior nares.

The skull was that of an adult man, 175 mm. long, 154 broad, 137 high, 515 in horizontal circumference, and with a capacity of 1650 cubic centimetres. It was brachycephalic, megacephalic, mesorhine, and mesognathous.

The skull was compared with the crania of Australians and Papuans, which are dolicocephalic, microcephalic, and prognathous; and it was pointed out that its affinities were not with these races but with the Malays.

Various methods of decorating preserved heads and skulls were then referred to. This communication will appear *in extenso* in the "Journal of Anatomy and Physiology," July 1880.

6. On an Ultra-Neptunian Planet. By Professor G. Forbes.

In continuation of researches communicated to the Royal Society of Edinburgh, 1880 (February 16), in which I gave the

probable position of an ultra-Neptunian planet, I have now to inform the Society that I have detected the existence of perturbations in the motion of Uranus, agreeing remarkably in character and period with those which would be produced by the new planet. These results are obtained from observations of Uranus extending over more than a century. The position of the planet, from this point of view, is found, from the first rough examination, to be the same as that given by me in my former memoir. This gives a means of determining the mass of the new planet. In this way I find it to be about the same as that of Saturn. I have also some reasons for believing that the following stars observed by Rümker, but stated by E. J. Cooper ("Markree Catalogue of Stars," vol. iv. p. 229) to be missing, are actually the new planet.

Number.	R.A.	N. Decl.
3320	10h. 37m. 24s. 203	10° 50′ 57″ 21
In Nach.	10 38 47 179	10 46
3372	10 44 24 365	9 55 52 58

I have not Rümker's Catalogue at hand at this moment to identify them.

The following star in Bessel's zones is also missing, and may be an observation of the planet. Cooper declares it to be missing.

Mag.	Zone.	R.A.	N. Decl.
9	280	9h. 51m. 59s. 82	16° 45′ 12″ 5

Monday, 7th June 1880.

J. H. BALFOUR, M.D., in the Chair.

The Chairman presented the Keith Medal for the Biennial period 1877–79, to Professor Fleeming Jenkin, for his Paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery," the second part of which was published in the Society's Transactions for 1878, and in doing so made the following remarks:—

Professor Jenkin has contributed several valuable papers to our Transactions.

In 1869 we had "The Practical Application of Reciprocal Figures to the Calculation of Strains on Framework," in which he exemplified in a very clear manner the mode of applying to important *statical* questions a beautiful principle, due in part to Rankine but mainly to Clerk-Maxwell.

The paper for which the award of the Keith Prize is now made is more thoroughly original, and may be roughly described as an extension of Maxwell's principle to the *kinetics* of machinery, where all parts move in one plane. It is entitled "The Application of Graphic Methods to the Determination of the Efficiency of Machinery." The first part was read to the Society in 1877, and the second in the following year. All three of these papers are in our Transactions.

Among his other contributions may be mentioned his application (in conjunction with Professor Ewing) of the Phonograph records to the "Harmonic Analysis of certain Vowel Sounds." This is an ingenious and elaborate piece of work, and shows us (among other things) within what wide limits the components of a sound may vary while it is still recognised by the ear as having a definite vowel quality.

Professor Jenkin, in handing you this medal I express, I am sure, the feelings of all the Fellows of the Society, when I say that we thank you heartily for the valuable contributions you have already sent to our Transactions, and that we look with confidence for an additional series.

Professor Jenkin then took the Chair.

The following communications were read:—

1. Non-Euclidean Geometry. By Professor Chrystal. (Plate XX.)

When I had the honour of being asked by the Council of the Royal Society to give the following address, I chose the subject partly because it had been brought under the notice of the fellows by my predecessor, Professor Kelland. His memoir was written comparatively early in the history of the subject; and he seems to have been but little acquainted with what others had done even up to the time at which

he wrote. Accordingly, although the subject is treated very ably in his paper, it is treated from only one point of view; and, indeed, one side of it is left out of sight altogether. The relation of the whole theory to the question of the origin and mutual independence of the axioms of geometry has been made much clearer of late, and I believed that some account of the more modern views might be of interest.

I am particularly desirous of bringing *pangeometrical* speculations under the notice of those engaged in the teaching of geometry. In discussing with schoolmasters the difficult problem of the reform of geometrical teaching, I have met with much enlightened and some unenlightened criticism. The former kind of criticism has convinced me that many teachers of mathematics will be glad to have this subject made more accessible; and I believe that a knowledge of what great mathematicians have thought on the subject would destroy criticism of the latter kind altogether.

It will not be supposed that I advocate the introduction of pangeometry as a school subject; it is for the teacher that I advocate such a study. It is a great mistake to suppose that it is sufficient for the teacher of an elementary subject to be just ahead of his pupils. No one can be a good elementary teacher who cannot handle his subject with the grasp of a master. Geometrical insight and wealth of geometrical ideas, either natural or acquired, are essential to a good teacher of geometry; and I know of no better way of cultivating them than by studying pangeometry.

The following sketch is addressed to those already familiar with Euclid's geometry. I have made no attempt to give a detailed account of modern researches, or to build up a systematic treatise. I have simply tried to give in a synthetic way a general idea of what is known in a certain department of a now very widely developed subject. In so doing I have used the materials and methods of Euclid as much as I consistently could, at some sacrifice of elegance, no doubt, but with obvious practical advantage.

I have not attempted to give any bibliographical details, for the simple reason that any one who wants them will find nearly all that can be desired in two papers by Mr Halsted in the first volume of the "American Journal of Mathematics."

On Pangeometry.

I know of no question possessing more interest for a thinker, and none of more importance for a mathematician, than the well-worn one of the origin of the axioms of geometry.

Passing over the discussions of mental philosophers, which, so far as I am acquainted with them, are of little mathematical or physical interest, we find two great modern contributions to this interesting subject; one by the mathematicians headed by Gauss, Lobatschewsky, Bolyai, and Riemann; the other by the physiologists represented by Helmholtz.

The mathematical investigators may be taken as representing the subjective side of the subject, the physiologists as representing the objective; although, in point of fact, Helmholtz, the personal representative of the latter, is a happy union of both classes of philosopher.

Any purely abstract science starts with certain data called definitions and axioms;* and of these materials reason builds the fabric of the science.

I do not intend to take up the question of the origin of axioms directly. On the contrary, I shall lay down axioms, and the only argument against me, so far, will be to prove the inconsistency of my conclusions with my premises, or with one another.

The absence of such inconsistency is what I mean by conceivability. I do not deny that other meanings may be attached to this word, and that the question of the conceivability of axioms might be profitably discussed from other points of view. We might discuss it as a purely personal question, each man to be judge and jury, or it might be granted, as I, for the most part in what follows, take it to be, that any axioms that can be made the foundation of a consistent reasoned system are given *à priori*. I suspect that this would be

* In Euclid's Geometry the functions of definition and axiom are not always clearly separated; at all events, some of his definitions serve purposes for which others are unfit, and this must be kept in view in what follows. With postulates I have at present nothing to do, as I am concerned solely with geometrical theorems. The *mixture* of problems with theorems is a peculiarity of Euclid's method for which there is no absolute necessity, and which is certainly inconvenient in an elementary text-book. Geometrical constructions are in a sense the applications of geometrical theory, and ought to be kept by themselves. The Society for the Improvement of Geometrical Knowledge have acted wisely, I think, in following this arrangement in their syllabus.

allowed by most of those who have considered the question of axioms in what I believe to be by far the most useful and effective way, viz., by examining and pushing the conclusions to be drawn from them to the utmost; and by investigating what change on these conclusions would be induced by varying one or more of the axioms themselves.

The question might also be approached from the side of experience. I take, for the sake of illustration, an instance which brings me at once to my subject. We have, by generalisation from experience, ideas more or less refined according to our individual physical education of a geometrical straight line, and of a geometrical point. Let us think, then, of two straight lines intersecting at a point, and let us ask ourselves, Can two such lines intersect again? Our first impulse is to answer no; but due consideration will show us that, in point of fact, experience does not settle the question. All we can say is that no one starting from the point of intersection of two straight lines has ever followed them by physical (say optical) observation to a second intersection. But then we must admit that, on our usual assumption that space is of infinite extent, and straight lines of infinite length, the distance through which any one has so followed them is, after all, relatively speaking, but an infinitely little way. Our assertion, therefore, that two straight lines never intersect again is merely an assumption, accordant, no doubt, with our limited experience, but otherwise unfounded, and certainly not of necessity involved in our idea of straightness, though we may superadd it thereto if we please. I recommend those who doubt this statement to begin by defining a straight line by a single geometrical property, which is not verbally equivalent to the assertion in question, and to attempt to prove it.

It may be well to remark here that the discussion of the properties of tridimensional space in reality divides itself into two parts:—first, what may the properties of space be conceived to be? *conceive* being understood in the sense above explained; second, what are the properties of space as we know, or think we know, them? The former question is a purely mathematical one; the latter is one in the main for the physicist or the mental philosopher, and the function of the mathematician in connection with it is to make clear what the question exactly is, and what alternatives are open for us. What the bearing

of modern mathematical research on this point appears to be, I shall endeavour to explain later on.

With these preliminary remarks in explanation, I now proceed briefly to sketch a system of geometry which, as to its foundations, differs from that of Euclid only in the alteration of one (or at most two) axioms. Its conclusions will be found to differ very materially from his, although this difference is merely in the way of wider generality, Euclid's geometry being contained as a particular case in what I shall, for distinction's sake, call Pangeometry.

The space which I shall consider is to be tridimensional. I appeal to the ordinary conceptions of

Point, Line or curve, Surface, Solid;

and, for the sake of the words, state that a point has no extension, a line is once extended, a surface twice, a solid thrice.

As a test of these distinctions, the idea of motion may be introduced. I cannot stop now to justify this, but merely remark that nothing is to be predicated concerning time.

Farther, space is to be uniform, in the double sense that it has no properties depending either on position or direction.

The great test of this last statement is congruency,* which I mention thus early, because it is the touchstone of geometry. Thus the statement that space has no properties depending on position, simply means that congruent figures exist, *e.g.*, that a solid of a certain size and shape can be carried from one part of space to another without alteration in either respect; and that two congruent figures can be conceived as separately existing in different parts of space. It is evident that all space measurement rests on congruency.

It is essential to be careful with our definition of a *straight line*, for it will be found that virtually the properties of the straight line determine the nature of space.

Our definition shall be that two points *in general* determine a straight line, or that in general a straight line cannot be made to pass through *three* given points.

It is important to notice the force of the phrase *in general*. This

* Two figures are said to be congruent when one can be placed on the other, so that every point of one shall coincide with a point of the other, and *vice versa*. The phrase *equal in every respect* is used in the same sense in most English editions of Euclid.

will be best understood from an illustration. We all know from the case of a three legged stool, if not from any more scientific source, that three points determine a plane. Yet not any three points; for, if the third foot were put in line with the other two, the one stool would be as unsafe a seat as the proverbial two. Yet again, and very near indeed to our case, two points on a sphere in general determine a great circle on it. But there are exceptions; a point and the diametrically opposite point do not determine a great circle, and yet it would be a good definition of a great circle to call it that line on a sphere which is in general determined when two of its points are given, no other condition being assigned.*

We recognise therefore that, although in general, any two points being taken, a line will thereby be determined, yet it may happen that, one point being taken, another point may exist which along with the first does not determine a straight line. The necessity for this admission appears when we consider space in which two straight lines have more than one point of intersection.

Here let it be mentioned, to avoid misconception, that it follows from our definition of a straight line, and from the uniformity of space (the test being congruency), that space is symmetrical round every straight line. This is at once an answer to those who say that pangeometry is merely an analogy drawn from the theory of surfaces of constant curvature.

A plane may be defined as Euclid defines it, and the conclusions drawn, that two intersecting lines, a point and a line, or a line passing through a given point and moving perpendicular to a given line, all in general determine a plane. The last form of definition of course presupposes the definition of a right angle.

Farther, we adopt all Euclid's definitions up to the definition of an

* It is interesting to notice that any curve already conditioned a number of times less by two than the whole number of conditions that completely determine it, fulfils in many respects the definition of a straight line, for any two points completely determine the curve. A very interesting particular case is that of a series of circles which always pass through a given fixed point. Such a series of circles may take the place of straight lines in many of Euclid's propositions. Most of the propositions as to congruency hold for them. The sum of the three angles of a triangle formed by three such circles is two right angles; the perpendiculars from the vertices of such a triangle on the opposite sides are concurrent; and so on, as is otherwise evident by the theory of inversion.

acute angled triangle, but reject in the meantime, at all events, all that follow in the first book.

Next we adopt Euclid's propositions concerning angles at a point, viz., I. 13, 14, 15; also the propositions as to congruency I. 4, 5, 6, 8, and the first part of 26, with a protest to the effect that in many cases his demonstrations are needlessly circuitous and difficult. All that is wanted for the demonstration of these propositions is the defining property of the straight line and the ordinary axioms and definitions as to equality.

Different Kinds of Space.

Before going farther, we must distinguish the different cases that may arise when we consider two intersecting straight lines.

1. They may never intersect again and be of infinite length (*i.e.*, each is non-re-entrant). Space which has this characteristic is called, for the present, hyperbolic space. We shall see, however, by and by that another case must be distinguished under this head, that, viz., of homaloidal or Euclidean space.

2. They may intersect again. Space having this characteristic is called elliptic space.

The simplest space of this kind is that in which a straight line returns into itself, so that the next point in which two straight lines intersect is the point in which they first intersected. In this kind of space, which I shall call single elliptic space, two straight lines intersect in only one point; and there is no exception to the statement that two points determine a straight line.

The next simplest case would be that in which two straight lines intersect a second time in a distinct point, and then re-enter at the next point of intersection which coincides with the original one. This might be called double elliptical space. I am not yet certain* whether the symmetry of space will allow us to carry this multiplicity

* I have not been able to find a definite settlement of this question by any of the great authorities on hyper space. Frischauf takes double elliptic space as the representative of elliptic space, and seems to hold that this is the only possible kind. Klein ("Mathematische Annalen," vi. 125) takes single elliptic space, and criticises Frischauf's view ("Fortschritte der Mathematik," viii. 313, 1876). Newcomb (Borchardt's Journ., lxxxiii. p. 293) professes himself unable to settle the question. If the notion of double elliptic space cannot be shown to be self-contradictory, then it would appear that the question becomes simply one of the choice of axioms. See note below, p. 661.

of elliptical space farther. In the meantime, I may remark that in a space of this second kind we must, as already explained, admit exceptions to the statement that two points determine a straight line.

In what follows I take single elliptical space as the representative of elliptical space generally, although on account of the non-existence of a closed surface of uniform positive curvature, on which a pair of geodetics intersect only once, the conclusions of the geometry of single elliptical space appear in some respects more bizarre than those of double elliptical space, whose planimetry is mirrored by the geodesy of a sphere.

It is obvious that Euclidean, or homaloidal, space is included in hyperbolic space as above defined. We shall afterwards show, however, that it may be regarded as a limiting case of elliptic space. It is therefore the transition case lying between the other two.

Sketch of the Geometry of Hyperbolic (Infinite) Space.

From the definition of this kind of space it is clearly infinite. Here I must insist on the distinction between infinite and unbounded, a distinction first brought into notice by Riemann. The uniformity of space necessarily involves the notion that it is unbounded, but by no means necessitates that it shall be infinite in extent; in fact, I shall point out directly that a single elliptical space is necessarily of finite extent.*

After the propositions relating to congruency already proved, the next fundamental proposition to be established is the following:—

In hyperbolic space the sum of the three angles of a rectilineal triangle cannot exceed two right angles.

The following proof of this proposition is due in substance to Bolyai. Legendre had given another, but he failed to see exactly the nature of the assumptions on which he founded.

ABC (fig. 1) is any triangle, O the middle point of BC, OD = OA; so that CD falls within the angle BCL. (Here we assume that a straight line is non-re-entrant, and that a pair of straight lines never intersect twice.) Then DOC ≃ †AOB; and ADC is equal in area to ABC, and

* An ellipse and a circle are unbounded but finite lines; a hyperbola is both unbounded and infinite.

† I adopt the sign ≃ used by continental writers for *congruent to*, or *equal in every respect to*.

has the sum of its angles the same, while the sum of A and D = BAC. Of these angles one is $\not>$, and the other $\not<$ than $\frac{1}{2}$ A. Taking the least of them, and bisecting the opposite side, we derive as before from ADC a triangle, still having the same area, and the same sum of all the angles, but in which the sum of two of the angles $\not> \frac{1}{2}$ A.

By a similar process we derive another triangle, still having the area and the sum of its angles unaltered, but in which the sum of two angles $\not> \frac{1}{2^2}$ A.

At last we get a triangle, in which the area is the same as at first, and the sum of the angles the same, but the sum of two of them $\not> \frac{1}{2^n}$ A, where n may be as great as we please; that is, in which the sum of two angles is as small as we please.

But the third angle can never be greater than 2R, hence the sum of the angles of the original triangle cannot be $>$ 2R.

It is to be noticed that this demonstration would fail if a straight line were re-entrant, or if two straight lines had more than one point of intersection.

Corollary.—If C′ be the external angle at C of the triangle ABC, then, since

$$A + B + C = 2R - \delta,$$

where R stands for a right angle, and δ is either zero or essentially positive, and

$$C + C' = 2R,$$

we have

$$C' = A + B + \delta;$$

That is, *the exterior angle of any triangle is not less than the sum of the two interior opposite angles.*

Of course it follows that *the exterior angle of any triangle is greater than either of the interior opposite angles; and that the sum of any two angles of a triangle is less than two right angles.*

We can now prove for hyperbolic space :—

That the greater side of every triangle has the greater angle opposite, and conversely.

That any two sides of a triangle are together greater than the third side.

Also *Euclid I.* 21.

Euclid I. 24 *and* 25.

Euclid I. 26 (*the second part*).

Also the usual propositions concerning the perpendicular and the obliques drawn from a given point to a given straight line.

The amount by which the sum of the three angles of a triangle falls short of 2R is called the *defect* of the triangle. This is the same as the excess of the sum of its exterior angles over 4R. If we take the latter statement of the definition, we may talk of the defect of any plane rectilineal figure. In forming the external angles of figures generally, we must go round, producing all the sides in the direction of our progress, assigning the positive or negative sign according as the angle is not or is re-entrant.

Thus in figure 2 the defect is

$$\alpha + \beta - \gamma + \delta + \epsilon - 4R.$$

Defining defect in this way, it is easy to prove that

The defect of any rectilineal figure is equal to the sum of the defects of any rectilineal figures of which it may be supposed to be composed.

Cor. Hence if one rectilineal figure lie wholly within another the defect of the former is not greater than that of the latter.

Hence follows at once the following important proposition:—

If the defect of any triangle whose sides are finite be zero, then the defect of every finite triangle must be zero.

For if ABC (fig. 3) be a triangle whose defect is zero, then, by applying to its sides three triangles, each congruent with itself, as shown in the figure, we evidently construct a triangle A′B′C′, having the same angles as ABC, and hence zero defect, each of whose sides is double a corresponding side in ABC. We may repeat this process with A′B′C′, and so on. Hence we may construct a triangle, having zero defect, large enough to contain within it any finite triangle whatever. But the defect of any triangle cannot be greater than that of a triangle within which it is contained, and the defect cannot be less than zero; hence the defect of every finite triangle must be zero, if the defect of any one finite triangle be zero.

Thus in *hyperbolic space*, as defined above, we are shut up to one or other of two alternatives. *Either the defect of a triangle is always positive or it is always zero.*

If we take the latter alternative, we get Euclidean or homaloidal space; and, from the defining property by which we have characterised it, we can prove Euclid's parallel axiom, and develop Euclid's geometry in his or any other equivalent manner.

Having separated out homaloidal space, let us now consider more closely hyperbolic space proper, in which the defect is always positive.

The fundamental proposition to be proved is the following.

The defect of a triangle (and consequently the defect of any plane rectilineal figure) is proportional to its area.

Various proofs of this proposition might be given. I select that which depends on the properties of the curves of equidistance from a straight line, because the intermediate propositions are the analogues in hyperbolic space to the propositions regarding parallels and parallelograms that are given in the latter part of Euclid's first book.

If in any plane perpendiculars of constant length be erected upon a given straight line, their extremities generate two curves which I shall call the equidistants, the two equidistants corresponding to a given length of the perpendicular may be called conjugate equidistants.

The equidistant is a self congruent line.

For if we take any piece AB (fig. 4) of the given line, and LM the corresponding piece of the equidistant, and if also A′B′ = AB and L′M′ be corresponding points to A′ and B′, then, if we place A′B′ on AB, L′ and M′ will coincide with L and M, and, if A′P′ = AP, Q′ will coincide with Q, and so on. Hence the piece L′M′ is congruent with the piece LM.

The equidistant is at every point at right angles to the generating perpendicular.

This is at once evident by considering two equal pieces (fig. 5) LP and LQ of the equidistant on either side of L, and the corresponding points A and B on the straight line, so that OA = OB. We have LOAP ≃ LOBQ, hence ∠ OLP = ∠ OLQ, each = R.

The equidistant in hyperbolic space is a curved line, concave towards the given line.

Let LQM (fig. 6) be a piece of the equidistant, LM a straight line cutting the perpendicular through P, the middle point of AB, in R. Then LRPA ≃ MRPB. Hence ∠ PRL = ∠ PRM = R, and the angles at P are each = R, therefore ALR < R.

But $\angle$ QLA = R, therefore LQ falls above LRQ, however small the distance AB may be; in other words, LQM is concave towards AB.

Every straight line terminated by a pair of conjugate equidistants to a given straight line is bisected by the given straight line, and makes equal alternate angles with the equidistants, &c.

If AB (fig. 7) be the given straight line, XP and YQ the equidistants, POQ the line terminated by the equidistants, then the proposition follows at once by observing that, if AP and BQ be perpendiculars to AB, then AOP $\simeq$ BOQ.

The common perpendicular to two conjugate equidistants is the least distance between them, the oblique distances are greater according as the angle they make with the perpendicular is greater, and the length of an oblique can be increased without limit.

It will be seen that conjugate equidistants are analogous to Euclidean parallels. The analogy may be carried much farther.

If equal arcs of two conjugate equidistants be joined towards the same parts by two straight lines, the figure so formed may be called a *hyperbolic parallelogram.*

A mixed triangle whose base is the arc of an equidistant, whose two remaining sides are straight lines, and whose vertex lies on the conjugate equidistant, may be called a *hyperbolic triangle.* The following propositions are then very easily proved.

The sum of the three angles of a hyperbolic triangle is 2R.

The opposite straight sides of a hyperbolic parallelogram are equal to one another; its diagonals bisect one another in a point on the straight line to which the equidistants that form its curved sides belong; and each diagonal divides it into two congruent hyperbolic triangles.

A series of propositions analogous to those of Euclid, Book I., 35–41, may be proved very easily; we have only to substitute hyperbolic parallelograms and triangles for ordinary parallelograms and triangles, and conjugate equidistants for parallels. In particular, we see (fig. 8) that

Two hyperbolic triangles CAOB, DAOB, which have for common base the arc AOB of an equidistant (and consequently have their vertices on the conjugate equidistant) are equal in area.

Hence follows at once that—

The rectilinear triangles CAB, DAB on the same chord of an equidistant, whose vertices lie on the conjugate equidistant, are equal in area and defect.

N.B.—the defect is 2 $\angle$ OAB in both cases. It is obvious that, if we join the middle points of the sides of any triangle, the extremities of its base lie on an equidistant to the line so drawn, and the vertex lies on the conjugate equidistant. Bearing this in mind, the properties of equidistants enable us to establish the following propositions :—

We can always construct an isosceles triangle whose base is equal to one side of a given triangle, and whose area and defect are the same as those of the given triangle.

*Given two triangles, we can always transform one or other of them into another of equal area and defect which has one of its sides equal to one of the sides of the remaining triangle.**

Hence two triangles that have the same area must have the same defect, and conversely, for we can transform them into a pair of isosceles triangles on the same base without altering either area or defect. It is obvious that two such triangles must be congruent if they are equal in area, and hence they must be equal in defect; and from what I have proved concerning the defect of composite figures, the converse follows with equal ease.

Hence *the area of a triangle is proportional to its defect.* Hence, ρ being a certain linear constant, characteristic of a hyperbolic space, and A the area of a rectilineal triangle of defect δ, we have

$$A=\rho^2\delta.$$

A great variety of very important conclusions can at once be drawn from this formula. I mention some of the most interesting.

Since $\delta=\frac{A}{\rho^2}$, if ρ be infinite, then $\delta=0$ for every triangle of finite area; in other words, homaloidal space is simply a hyperbolic space whose linear constant is infinite. This conclusion may be looked at from another, but mathematically equivalent, point of view. Let us imagine a hyperbolic space of given linear constant ρ.

* I leave the reader to consider and settle for himself whether a simpler proposition than the above could be established. In particular he should consider the following problem in hyperbolic geometry:—"To construct an isosceles triangle of given area on a given base."

If we take a region in this space whose greatest linear dimension is an infinitely small fraction of ρ, then the defect of every triangle within that region will be infinitely small, and its geometry will not differ sensibly from that of a homaloidal space. This is often expressed by saying that hyperbolic space is homaloidal in its smallest parts.

It appears, therefore, that, even in hyperbolic space, Euclid's planimetry will apply to infinitely small figures. For instance, the ratio of the circumference of a circle to its diameter will be $\pi = 3{\cdot}14159 \ldots$ (the ordinary transcendental constant), when the diameter is made infinitely small. We may, therefore, if we please, measure our angles in radians (circular measure), and in fact use all the formulæ of homaloidal plane trigonometry, if proper restrictions be observed.

It should also be noticed that the existence of this length ρ related to the space, but not *directionally* related, suggests the possibility of explaining the properties of tridimensional space by subsuming it in a space of four or more dimensions. I have not chosen to enter into speculations of this nature, partly because their development has been entirely analytical hitherto; and partly because, so far as I can see at present, it may be justly contended that the conceivability of hyperspace of three dimensions rests on different grounds from that which we must necessarily assume when we attempt to add another dimension. In this, however, I may be but one of those whom Gauss playfully called Bœotians.*

* Before leaving this part of the subject, I may mention the curious solution of the problem of dividing a plane in hyperbolic space into a network of regular polygons.

If n be the number of sides of each polygon, p the number of polygons round a point of the network, A the area of each of the n-gons, then

$$A = n\pi\rho^2\left(1 - \frac{2}{n} - \frac{2}{p}\right),$$

with the condition $\frac{1}{n} + \frac{1}{p} < \frac{1}{2}$.

Suppose, for instance, we wish to divide a plane into squares, *i.e.*, regular four-sided figures. Then $n=4$. If $p=4$, *i.e.*, if the angles of the square be right angles, $A=0$, which does not, strictly speaking, give a solution. The next case is $p=5$, so that $A=\frac{2}{5}\pi\rho^2$ is the area of the smallest finite square with which we could pave a plane floor. Of course there are an infinite num-

Theory of Parallels.

If O (figs. 9 and 10) be any point outside a line, P any point in it to the right of the foot of the perpendicular, then the limiting position of OP, when P is moved in the direction DI to the right, without limit, is called the parallel through O to DI. The corresponding limiting line on the other side of OD is called the parallel through O to DI′.

Thus

$$\begin{aligned} &OK \parallel DI \\ &OK' \parallel DI'. \end{aligned}$$

It is obvious, from the uniformity of space, that OK and OK′ make equal angles with OD. Whether they are parts of the same line or not, remains to be seen.

As P moves off along DI the angle at P diminishes without limit.

This is easily shown (fig. 10) by taking $PP_1 = OP$, $P_1P_2 = OP_1$ and so on *ad. inf.*

In homaloidal space the parallel to DI through O is the perpendicular to DO at the point O: for the sum of the three angles of the triangle DPO is always 2R, and P diminishes without limit, hence the angle at O approaches nearer to R than by any assignable quantity.

Thus in homaloidal space the two parallels OK, OK′ are parts of the same straight line, and all the lines through O cut IDI′, except the parallel, which may be said to cut it at an infinite distance. In the language of modern geometry there is but one point at infinity on the line IDI′.

In hyperbolic space there are two parallels through a given point to a given straight line.

For as we move P away from D the area of ODP, and consequently its defect, constantly increases, but the angle OPD constantly diminishes, hence the angle at O can never exceed a certain angle which is less than a right angle.

It follows, therefore, that if we take any line IDI′ and any external point O, we must classify the lines through O as follows :—(1) intersectors, (2) non-intersectors, (3) two parallels.

ber of solutions, the angles of the squares becoming less and their area greater as p increases. The area of the greatest possible square tile that we could use would be $2\pi\rho^2$, but the lengths of the sides would be infinite.

In figure 11 KOL′ and K′OL are the two parallels; all lines lying in the angles KOL, K′OL′, are non-intersectors, all those lying in KOK′, LOL′ are intersectors. The fact that in hyperbolic space there are two parallels through a given point to a given straight line is expressed in modern geometry by saying that in hyperbolic space a straight line has two distinct real points at infinity.

After what has been laid down, the following propositions either are immediately evident, or can be proved with very little trouble.

If a line is parallel to another at any point, it is so at every point of itself.

Parallelism is mutual.

Lines which are parallel to the same line are parallel to one another.

Lines that are parallel continually approach one another on the side towards which they are parallel.

Non-intersectors in the same plane have a minimum distance, which is the common perpendicular.

The angle which a parallel through O to L makes with the perpendicular on L is called the parallel angle.

The parallel angle is a function of the length of the perpendicular, increasing when the perpendicular diminishes.

If θ be the angle, p the length of the perpendicular, then it may be shown by methods which I shall presently explain that

$$\tan \tfrac{1}{2}\theta = e^{-\frac{p}{\rho}},$$

$$\text{When } p = 0,\ \theta = \frac{\pi}{2}\,;\ \text{when } p = \infty,\ \theta = 0.$$

Geometry of Elliptic Space.

For simplicity I take single elliptic space, but there will be no difficulty in modifying what follows so as to make it apply to double elliptic space.

In single elliptic space every straight line returns into itself; and two straight lines intersect in only one point. Thus, starting from any point P, and proceeding in any direction continuously, we at last return to the point P; the length L travelled over in this process is called the length of the *complete straight line.*

It is obvious that in single (as well as in double) elliptic space

two intersecting complete straight lines enclose a plane figure. Such a figure I call a *biangle.*

Two biangles are congruent when their angles are equal. All complete straight lines are of the same length, and all the straight lines emanating from the same point intersect in the same second point.

These propositions are all equivalent to one another, and are equally true for single or double elliptic space. The last of them is a mere truism for *single* elliptic space. The following demonstration, which holds good for single or double elliptic space, may help to render the matter clearer.

Let APBQA A′P′B′Q′A′ (fig. 12) be two biangles having the angles A and A′ equal. If A′B′ be placed on AB so that A lies on A′, and A′P′ along AP, then A′Q′ will lie along AQ, since the angles at A are equal; hence by the fundamental property of a straight line APB and A′P′B′ must wholly coincide, and AQB and A′Q′B′ must wholly coincide; and hence B′ must fall on B. It is to be noticed that the biangles are multiply congruent.

Next, suppose AKA′, AK′A′ (fig. 13) to be any pair of intersecting straight lines. Let AL bisect the angle A and cut the lines in J and J′. Since AJ and AJ′ are equiangular biangles, they are congruent; from this it follows at once that J and J′ must coincide with each other, and therefore each with A′. Hence the bisector of the angle A passes through A′; and it and AKA′ and AK′A′ are all of equal length. We may next bisect either of the halves of A, and so on; and we may double any of the angles thus obtained as often as we please. Hence the propositions stated above are completely proved. The length L of a complete straight line is therefore an absolute linear constant which characterises an elliptic space.

In single elliptic space the least distance between two points can never be greater than $\frac{1}{2}L$, and the greatest distance can never be greater than L.

This is obvious, since the whole length of a complete straight line through the two points is L.

If we consider the plane determined by two intersecting straight lines AOA, BOB, and if we pass from O along OA through a length L, we return to O, but find ourselves on the opposite side of the plane to that from which we started, and only arrive at the same point O

on the same side as before by travelling once more through a length L.

This curious conclusion is an immediate result of the fact that straight lines are re-entrant and intersect only once. (In double elliptical space the apparent anomaly does not occur on account of the double intersection.)

The best way of representing the thing to the mind that I can think of is to imagine a rigid body composed of three rectangular arrows Ix, Iy, Iz (fig. 14). Ix slides along OA; Iy passes through a ring which slides on OB (being long enough never to slip out); Iz is, of course, determined in position when Ix and Iy are fixed in any positions.

In starting from O, let Ix and Iy be horizontal and Iz vertical; then slide Ix along OA. Ix will at last return along A′O. The ring will return along B′O. It is obvious, therefore, that, at our first return to O, Iz must be downwards, for, since the system of arrows is rigid, one who plants himself with feet at I, head at z and looks along Ix must see y to his left as he did at starting.

It is obvious that during the journey Iy as well as Iz has rotated through 180°, a repetition of the process rotates both through 180° more, and then everything is as before.

If we cause a complete straight line of length L to revolve through 360°, always remaining perpendicular to a given line, it will sweep out the two sides of a *complete plane.*

It follows at once, therefore, that the area of a complete plane, taking into account both sides, is finite, and the same for every complete plane. This I shall call P in the meantime. We also see, in accordance with what was proved before, that the two sides of the complete plane are not distinct, since we can pass continuously upon the plane from a point on one side to the same point on the other side.

Those who find difficulty in realising this property of the plane in single elliptic space should take a ribbon of paper, twist it through 180°, and then gum the ends together. A surface is thus formed which has the property that one can trace a continuous line upon it from a point on one side to a point exactly opposite on the other side.

After what has been laid down the following propositions are obvious.

They are given by Newcomb in an extremely interesting article to which reference was made above. I arrange them in the order which best suits what has gone before.

All the perpendiculars in a given plane to a given straight line intersect in a single point, whose distance from the straight line is $\frac{1}{2}L$.

Conversely, the locus of all the points at a distance $\frac{1}{2}L$ *on straight lines passing through a given point in a given plane is a straight line perpendicular to all the radiating lines.*

The fixed point is called the *pole*, and the straight locus its *polar*.

If we cause the given plane to rotate about the polar the pole describes a straight line which may be called the conjugate of the given polar.

The relation of these two lines is mutual, every point on one being at a distance $\frac{1}{2}L$ *from every point on the other.*

Without dwelling farther upon propositions of this kind, I proceed at once to establish the fundamental proposition concerning the sum of the angles of a plane triangle. I might follow a course like that adopted for hyperbolic space, but a much simpler method suggests itself at once as applicable to finite space.

In the first place, since a complete plane is generated by the revolution of a complete straight line through 360°, it follows that the area of a biangle whose angle is A° is $\frac{A}{360}P$.

In figure 15 let ABC be any triangle. Produce the sides to form biangles. Each of the biangles departs from the vertex on the upper side of the plane and returns to the vertex on the lower side. To make this clear areas in the neighbourhood of ABC in the figure are shaded with vertical lines when reckoned on the upper and with horizontal lines when reckoned on the lower side of the plane. A glance will show that if we take the three biangles they overlap the triangle ABC thrice, and that the rest of the plane is covered every where once on one side or the other, but nowhere on both sides. Hence, Δ denoting the area of the triangle, we have

$$\frac{A}{360}P + \frac{B}{360}P + \frac{C}{360}P = \tfrac{1}{2}P + 2\Delta$$

$$\Delta = \frac{A+B+C-180}{360}P.$$

If, therefore, we define $A^\circ + B^\circ + C^\circ - 180^\circ$ as the *excess* of the triangle, we have the proposition that—

The excess of every triangle is positive, and is proportional to its area.

The conclusions drawn above (p. 650) for hyperbolic space follow here, *mutatis mutandis.* In particular, we see that we may apply Euclidean planimetry to infinitely small figures. On this remark we can, as will be done later, found a system of planimetry for elliptic space, and determine P. The result is $P = \frac{4L^2}{\pi}$. Hence, writing ρ for $\frac{L}{\pi}$, and ϵ for the radian measure of the excess, we have

$$\Delta = \rho^2 \epsilon$$

where ρ is a linear constant characteristic of the elliptic space.

It is easy after what has now been established to work out the propositions corresponding to Euclid's first book. The conclusions will, of course, be subject to certain modifications, but these are easily found. I may mention in particular that the propositions concerning the curves of equidistance already given for hyperbolic space, hold with very slight modification for elliptic space, the main difference being that the equidistants are convex instead of concave to the given straight line.

Theory of Parallels.

In elliptic space there is, of course, no such thing as a parallel, because there are no infinitely distant points on a straight line.*

If O (fig. 16) be a point outside the line IDI′; then it is easy to see that the two segments of the perpendicular from O are respectively the least and greatest distances from the given line. If OD be the least distance, then, as OP, starting from OD, revolves about O, OP continually increases, until it has rotated through 180°, and then it is at its maximum, after which it decreases again.

It can easily be shown that, as OP revolves from OD, the angle OPD decreases, until OP is perpendicular to OD, and then OPD is at its minimum value. After that, as may be easily shown by producing the line backwards through O, the angle again increases.

* In the language of modern geometry the points at infinity on a straight line in elliptic space are imaginary.

The line OI, perpendicular to OD, is all that there is in elliptic space to represent a parallel through O to the line I′DI.

General Conclusions.

If I have succeeded in my attempt to explain the results of modern research concerning the axioms of geometry, it will be apparent that, even if we overlook the possibility of space being non-uniform, in the sense of having properties depending on position and direction, it is still possible to develop three self-consistent kinds of geometry —the hyperbolic, the homaloidal, and the elliptic. It is impossible, it appears to me, to say on *à priori* grounds that any one of these is more reasonable than the others. If, therefore, *à priori* ground is to be sought for the axioms of geometry, such tests of its firmness "as the inconceivability of the opposite" and others like it are not to be relied upon. They are merely an appeal to ignorance.

If, on the other hand, we view the question from the side of experience, three alternatives are open to us. We may hold that space is homaloidal and therefore infinite. In this case we extend to the infinite part of space which we do not know the results of our experience of the finite part of it that we do know.

Again, we may hold that space is hyperbolic and therefore infinite. In this case experience teaches us that the radius of the sphere of our experience is infinitely small compared with the linear constant of space; for Lobatschewsky calculated from astronomical observations the sum of the three angles of triangles whose smallest sides were about double the distance of the earth from the sun, and found that the difference from two right angles was not greater than the probable error of observation.

Lastly, we may suppose that space is elliptic and therefore finite, in this case we must admit that our experience extends to but an infinitely small fraction of its whole extent, since no sensible excess can be found in the largest triangles with which we are acquainted.

Before leaving this subject, it may be well to illustrate with some care what is meant by the words finite and infinite as I have used them. They have, of course, a purely relative meaning. In the geometry of homaloidal space no distinction can be built on the relative dimensions of figures apart from their form. Owing to the

existence of similar figures, the geometrical experience of a cheese mite in homaloidal space would not be different from that of a being one of whose habitual walking steps was from the sun to the dog star.

In hyperbolic or elliptic space the case is otherwise. In either of these two kinds of space we might divide intelligent beings into two classes according to their bodily dimensions. We might have a race of micranthropes, whose bodily dimensions and the radius of whose sphere of experience were infinitely small compared with the linear constant of space. For instance, if the space were elliptic, the world of the micranthropes would be but an infinitely small fraction of the elliptic universe. It must be noticed, however, that from the point of view of a micranthrope, his world need not be a prison-house by any means, for he would compare it not with the linear constant of universal space, of whose magnitude he must necessarily be ignorant, but with some arbitrary standard such as the length of his own arm, and so considered his world would to him be infinite, if we only suppose him small enough. Again, we might have a race of macranthropes, whose bodily dimensions were comparable with the linear constant of space. In the case of an elliptic and finite space, we could, of course, conceive one of these himself so great that there would not be room enough in the universe for another as great.

The geometry of the micranthropes would, of course, be homaloidal. The axioms of Euclid would appear to them strictly in accordance with experience, and, although they lived in part of an elliptic or hyperbolic space, their prejudices would render the conceptions of the general properties of such a space as difficult to them as they are to us. On the other hand, the geometry of the macranthropes would be elliptic or hyperbolic, as the case might be. A hyperbolic macranthrope would, of course, be familiar with the fact that the defect of a triangle diminishes as its area diminishes. If he were a mathematician he would be aware of the relation of proportionality, and might speculate concerning triangles of zero defect, much as we do about absolute zero of temperature. If Euclid's geometry were to fall into the hands of an instructed macranthrope, he would very likely regard it as the production of some macranthropic lunatic, who had meditated on the fact that the defect of a triangle diminishes with its area, until he had so far lost his wits as to commit the *ὑστερον προτερον* of discussing the construction of an equilateral triangle

before proving that when two straight lines cut one another the vertically opposite angles are equal!

Appendix on the Trigonometry of Elliptic and Hyperbolic Space.

The following appears to me to be the simplest, and at the same time the most instructive way of establishing the Trigonometry of Elliptic and Hyperbolic Space.

The method might, indeed, by assuming proper axioms, be made to take the place of the preceding synthesis. As it is, I shall base it upon the results of that synthesis. What I shall want are mainly the propositions concerning the excess or defect of plane triangles, and the conclusion founded on them that homaloidal trigonometry may be applied to figures, all of whose dimensions are infinitely small compared with the linear constant of space.

Let KA and LB (fig. 17) be two straight lines in the same plane at an infinitely small distance apart. They may be either non-intersectors, whose minimum distance d is infinitely small, or intersectors which make a very small angle a with each other at their point of intersection.

Let KL, AB, CD be lines making equal angles with KA and LB; and let $KA = LB = r$, $AC = BD = dr$, $AB = D$, $CD = D + dD$, where dr is infinitely small compared with r, dD infinitely small compared with D; D of course is infinitely small compared with ρ, the linear constant of space.

Further, let $\angle LBA = \angle KAB = \frac{\pi}{2} - \theta$, and $\angle LDC = \angle KCD = \frac{\pi}{2} - \theta - d\theta$.

Since all the dimensions of ABDC are infinitely small compared with ρ, we may apply Euclidean trigonometry. Draw Bm parallel to AC. Then $\angle ABm = \frac{\pi}{2} - \theta$, $AB = Cm$, and $Dm = dD$.

$$2 \sin \tfrac{1}{2} DBm = 2 \sin(-\theta) = \frac{dD}{dr};$$

$$\theta = -\tfrac{1}{2} \frac{dD}{dr} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Now the excess of $ABDC = 2\left(\frac{\pi}{2} + \theta\right) + 2\left(\frac{\pi}{2} - \theta - d\theta\right) - 2\pi = -2d\theta$; and its area $= Ddr$. Hence

$$Ddr = \rho^2\epsilon = -2\rho^2 d\theta.$$

which gives

$$2\frac{d\theta}{dr} + \frac{D}{\rho^2} = 0.$$

Whence by (1)

$$\frac{d^2D}{dr^2} + \frac{D}{\rho^2} = 0. \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

This is the equation for Elliptic Space ; that for Hyperbolic Space is of course

$$\frac{d^2D}{dr^2} - \frac{D}{\rho^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2')^*$$

From the equation (2) we get at once

$$D = \rho\alpha \sin\frac{r}{\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

r being measured from the intersection of the lines, and the constants of integration determined by the condition

$$D = \alpha r$$

when r is infinitely small compared with ρ, which of course includes the condition $D = 0$ when $r = 0$.

The corresponding formulæ for Hyperbolic Space are

$$D = \rho\alpha\left(\frac{e^{\frac{r}{\rho}} - e^{-\frac{r}{\rho}}}{2}\right)$$

$$= \rho\alpha \sin h\frac{r}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

for a pair of intersectors ; and

$$D = d\left(\frac{e^{\frac{r}{\rho}} + e^{-\frac{r}{\rho}}}{2}\right)$$

$$= d \cos h\frac{r}{\rho} \quad . \quad . \quad . \quad . \qquad (5)$$

for a pair of non-intersectors, r being measured in the one case from the intersection, in the other from the points of minimum distance.

From the formulæ (3), (4), and (5) all the trigonometry of Elliptic and Hyperbolic Space can be deduced most readily. I append one or two applications, and select for my purpose important formulæ, but anything like a complete development would be out of place here.†

* The differential equations (2) and (2′) contain all the metrical properties of elliptic and hyperbolic space. (2) suggests that a pair of straight lines diverging at a small angle from a point might intersect again in distinct points any number of times. The proposition proved above for elliptic space generally, that all the lines radiating from any point intersect in the same second point, seems, however, to compel us to conclude that at the point where any line intersects another for the second time, it must return into itself; for a line can be brought by continuous rotation into coincidence with its prolongation, hence we must reach the same second point of intersection in whichever direction we proceed from the first point. I can see no way out of this at present; and if there is none, it would appear that we cannot get beyond double elliptic space, even if we can consistently get so far.

† I may refer the reader to Frischauf, "Elemente der Absolute Geometrie," Leipzig, 1876 ; Lobatschewsky, Crelle, xvii. p. 295 ; Klein, Annalen der Mathematik, iv. p. 573, vi. p. 112, &c. ; Cayley, Annalen der Mathematik, v. p. 630.

Area of Complete Plane and Total Volume of Elliptic Space.

The area of a biangle having the infinitely small angle α is

$$\rho\alpha\int_0^{\pi\rho}\sin\frac{r}{\rho}\,dr=2\rho^2\alpha.$$

Hence
$$P=4\pi\rho^2=\frac{4L^2}{\pi}\quad . \quad . \quad . \quad . \qquad (6)$$

From this result we can deduce very easily the total volume S of elliptical space (single). The locus of the most distant points on the radii through any point of space is a plane. Suppose this plane divided up into infinitely small regular quadrilaterals (squares) of side k. The volume dS contained by four radii drawn to the vertices of one of these figures is

$$dS=k^2\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}\rho}\sin\frac{2r}{\rho}\,dr=\frac{\pi}{2}k^2\rho\,;$$

Hence
$$S=\frac{P}{2k^2}dS=\frac{P\rho}{4},$$

$$=\pi^2\rho^3,$$

$$=\frac{L^3}{\pi}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

This curious result can also be obtained by calculating the volume swept out by a complete plane rotating through 180° about any line in it.

Formulæ for Right-Angled Triangles.

Let ACB (fig. 18) be a triangle right angled at C. Let BA$b=d$A, B$b=da$. CbA$=$B$+d$B. If Bm be perpendicular to Ab, then $bm=dc$.

We have at once by (3)

$$\sin B\,da=\rho\sin\frac{c}{\rho}\,dA\quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

Also
$$dc=da\cos B\quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

Calculating the area BAb in the two different ways we get

$$\int_0^c D\,dc=\rho^2\epsilon.$$

Whence

$$dA\rho^2\left(1-\cos\frac{c}{\rho}\right)=\rho^2(dA+dB)$$

i.e.,
$$dB=-\cos\frac{c}{\rho}\,dA\quad . \quad . \quad . \qquad (10)$$

Whence $$dc \tan B = -\rho \tan \frac{c}{\rho} dB \quad . \quad . \quad . \quad . \quad (11)$$

From these equations we get successively:

$$\sin B \sin \frac{c}{\rho} = \sin \frac{b}{\rho} \quad . \quad . \quad . \quad . \quad . \quad \text{I.}$$

$$\cos \frac{a}{\rho} \cos \frac{b}{\rho} = \cos \frac{c}{\rho} \quad . \quad . \quad . \quad . \quad . \quad \text{II.}$$

$$\sin A \cos \frac{b}{\rho} = \cos B \quad . \quad . \quad . \quad . \quad . \quad \text{III.}$$

$$\tan \frac{b}{\rho} \cot \frac{c}{\rho} = \cos A \quad . \quad . \quad . \quad . \quad . \quad \text{IV.}$$

For hyperbolic space we get in like manner

$$\sin B \sin h \frac{c}{\rho} = \sin h \frac{b}{\rho} \quad . \quad . \quad . \quad . \quad . \quad \text{I}'.$$

$$\cos h \frac{a}{\rho} \cos h \frac{b}{\rho} = \cos h \frac{c}{\rho} \quad . \quad . \quad . \quad . \quad . \quad \text{II}'.$$

$$\sin A \cos h \frac{b}{\rho} = \cos B \quad . \quad . \quad . \quad . \quad . \quad \text{III}'.$$

$$\tan h \frac{b}{\rho} \cot h \frac{c}{\rho} = \cos A \quad . \quad . \quad . \quad . \quad . \quad \text{IV}'.$$

The reader will observe that these are simply the formulæ included in Napier's rules for right-angled spherical triangles. The only modification being that in hyperbolic space hyperbolic functions take the place of circular functions. In other words, the trigonometry of single elliptic space is identical with the geodetic trigonometry of a sphere, although it would not be correct to say that the planimetry of single elliptic space is identical with the geodesy of a sphere.

For hyperbolic space the analogue is the pseudo-spherical surface of Beltrami.

Parallels.

As an illustration of the application of the above formulæ to parallels, I shall find the parallel angle in hyperbolic space.

Taking formula IV′, if we make B move off to an infinite distance, then AB becomes the parallel to CB. A is then the parallel angle corresponding to b. Now since $c = \infty$ we have

$$\cot h \frac{c}{\rho} = 1,$$

therefore

$$\cos A = \tan h \frac{b}{\rho} = \frac{e^{\frac{b}{\rho}} - e^{-\frac{b}{\rho}}}{e^{\frac{b}{\rho}} + e^{-\frac{b}{\rho}}} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Whence $$\tan\frac{A}{2}=e^{-\frac{b}{\rho}},$$

the relation stated above (p. 653).

Non-Intersectors.

As an example of the trigonometry of non-intersectors, I select the following formulæ, the proof of which I leave to the reader.

If KA and LB be two non-intersectors, K and L the points of least distance, $KA=LB=r$, $KAB=LBA=\phi$, $KL=d$, $AB=D$.

Then $$\sin h\frac{D}{2\rho}=\sin h\frac{d}{2\rho}\cos h\frac{r}{\rho} \quad . \quad . \quad . \quad . \qquad (13)$$

$$\sin\phi=\frac{\cos h\dfrac{d}{2\rho}}{\cos h\dfrac{D}{2\rho}} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

The results of (6) to (11) are given by Newcomb (Borchardt, lxxxiii. p. 293) mostly without demonstration. He assumes formula (3) as one of the axioms on which he bases his synthesis. Although I have read most of the original literature on the subject, I am more immediately indebted to Newcomb and Frischauf for the materials of the foregoing sketch.

2. Note on the Theory of the "15 Puzzle." By Professor Tait.

[After this note had been laid before the Council, the new number (vol. ii. No. 4) of the "American Journal of Mathematics" reached us. In it there are exhaustive papers by Messrs Johnston and Story on the subject of this American invention. The principles they give differ only in form of statement from those at which I had independently arrived. I have, therefore, cut down my paper to the smallest dimensions consistent with intelligibility.—P. G. T.]

The essential feature of this puzzle is that the circulation of the pieces is necessarily in rectangular channels. Whether these form four-sided figures, or have any greater (*even*) number of sides, the number of squares in the channel itself is always even. (This is the same thing as saying that a rook's re-entrant path always contains an even number of squares. This follows immediately from the fact that a rook always passes through black and white squares alternately. The same thing is true of a bishop's re-entering

path, for it is a rook's upon a new chess-board formed by the alternate diagonals of the squares on the original board.) That there may be circulation in the channel, one of its squares must be the blank one.

Hence an *odd* number of pieces lies along the channel, and, therefore, when they are anyhow displaced along it, so that the blank square finally remains unchanged, the number of interchanges is essentially *even*.

Thus to test whether any given arrangement can be solved, all we need know is how many interchanges of two pieces will reduce it to the normal one. If this number be even, the solution is possible. To find the number of interchanges, we have only to write in pairs the numbers occupying the same square in each arrangement, and divide them into groups, such as $\begin{matrix} a & b & c & d \\ b & c & d & a \end{matrix}$, which form closed cycles. Here there are *four* pairs in the group, which correspond to *three* interchanges, because $\begin{matrix} a & b \\ b & a \end{matrix}$ is one interchange.

Dr Crum Brown suggests the term *Aryan* for the normal arrangement, with the corresponding term *Semitic* for its perversion. Similarly *Chinese* would signify the Aryan rotated right-handedly through a quadrant, and *Mongol* Semitic rotated left-handedly through a quadrant.

Now it is easily seen that Aryan is changed into Semitic, and Chinese into Mongol, or *vice versâ*, by an odd number of interchanges. Similarly Aryan and Mongol, and Semitic and Chinese, differ by an even number of interchanges.

Hence any given arrangement must be either Aryan or Semitic. The former can be changed into Mongol, the latter into Chinese.

Unless the 6 and 9 be carefully distinguished from one another every case is solvable, for if it be Semitic the mere turning these figures upside down effects one interchange and makes it Aryan.

The principle above stated is, of course, easily applicable to the conceivable, but scarcely realisable, case of a rectangular arrangement of equal cubes with one vacant space.

3. On the Constitution of Adult Bone-Matrix and the Functions of Osteoblasts. By De Burgh Birch, M.B., Demonstrator of Physiology in the University of Edinburgh.

(*Abstract.*)

By tearing thin sheets from the surface of decalcified bones, Sharpey first demonstrated the lamellar nature of bone, and also that these lamellæ have a fibrous structure.

The fibres, consisting of two sets crossing each other, were thought to be interwoven in each lamella, and contiguous fibrous laminæ to be separated from each other by a homogeneous ground substance in which the lime salts were mainly situated; alternations of fibrous and homogeneous layers giving rise to the appearance of lamellation.

From the researches of V. Ebner and my own investigations, the matrix is undoubtedly throughout fibrous, the lamellar appearance being due to an alternation of layers in which the directions of the fibres differ.

By digesting with trypsin sections of decalcified bone, the white fibrous tissue elements of which had been rendered indigestible by protracted treatment with chromic acid, the interfibrillar substance was entirely removed.

In sections thus treated, an alternating band and dot series obtained, where one set of lamellæ was cut longitudinally and the other transversely.

In Haversian systems the entire system in section presented the same appearance, or the lamellation was perceptible only over certain parts, this difference being due to the fact that in some cases the fibres are arranged spirally, whilst in others they run parallel and transversely to the long axis.

Trypsin digestion further caused the isolation of lacunar membranes with their tubular prolongations the canaliculi; these were found united to similar membranes lining the interior of Haversian canals.

Such membranes were also met with in the substance of the

Haversian system, indicating the probable occurrence of periods of cessation, in the deposition of the systemic lamellæ, of some duration.

The situation of the lacunæ was found to be almost universally in the thickness of a lamella.

The nature of the lacunar membrane is not elastic.

Functions of Osteoblasts.—For the ordinary classification of bone formation, it seems to me that the following is a subdivision more in accordance with observed facts. Bone is produced under conditions where

1. There is preformed fibrous tissue,
2. Where no preformed tissue occurs.

Subperiosteal bone formation comes under the first, and under the second, the production of bone upon cartilage spicules and in the excavations for Haversian canals.

There are situations where bone grows at the expense of tendons inserted into them; the level of ossification is indicated by a regular and rapid proliferation of the connective tissue corpuscles, the resulting corpuscles becoming lacunar cells and secreting the calcareous matrix, the fibrous tissue of the tendon becoming that of the osseous matrix.

Subperiosteally the fibrous tissue of the osseous matrix is preformed as the reticulum of the periosteum. This reticulum is produced by connective tissue corpuscles of the periosteum; other protoplasts secrete the interfibrillar calcareous cement.

From these appearances and others observable upon the spicules of cartilage in subepiphysal bone production, it seems justifiable to ascribe the production of the fibrillar elements on the one hand, and the interfibrillar calcareous cement with lacunar and canalicular membranes on the other to two sets of protoplasts, which I would designate as "spinners and plasterers."

4. On two unrecorded Eggs of the Great Auk (*Alca impennis*) discovered in an Edinburgh Collection; with remarks on the former existence of the bird in Newfoundland. By Robert Gray. [Specimens exhibited.]

The two eggs of the Great Auk which I now exhibit were bought in Dowell's Auction Rooms rather more than a month ago, and formed part of a collection of "birds' eggs, shells, and other natural history specimens" which was disposed of among a lot of miscellaneous property belonging to a legal gentleman of this city. This small collection of eggs had been in the possession of the owner for about thirty years, and the two eggs in question had been purchased by his father from another collector in Edinburgh—a Mr Little—in whose possession it is thought, from collected and trustworthy evidence, the specimens had been at least other thirty years. These eggs, therefore, have probably not changed hands more than once during a period of fifty or sixty years. The present owner of the specimens, Mr Small, animal preserver, George Street, purchased the lot at the sale for £1, 12s., and has since taken great pains to establish the few facts I have stated regarding their history.*

The chief interest in the eggs of the Great Auk arises from the circumstance of the bird itself being now regarded as an extinct species. In the early part of the present century it was reckoned a very rare bird, although it appears to have lingered in Scotland, where it was confined to the Orkney Islands† and St Kilda,‡ until 1821; in Ireland§ until 1834; in Newfoundland,|| which country may be regarded as having been at one time the stronghold of the species, until probably 1837; and in Iceland¶ until 1844, since

* The two eggs have since been sold by auction in London; one specimen realising £100, and the other £107, 2s. Both are now in the collection of Lord Lilford.

† Montagu, "Ornithological Dictionary," Appendix to Supplement, 1813.

‡ Fleming, "Edinburgh Philosophical Journal," vol. x. 1824.

§ Thomson, "Birds of Ireland," vol. iii. p. 238.

|| Audubon, "Orn. Biog." 1838.

¶ Newton, "Ibis," 1861, pp. 390–392.

which year all traces of the living bird have been lost. Very great interest has consequently been felt by all classes of naturalists in everything that relates to the Great Auk; and the discovery of two specimens of its egg, in addition to those already recorded as existing in collections, is an event which is sure to create some degree of excitement, not only among egg collectors, but among scientific ornithologists in all parts of the world.

Some years ago, in writing a history of this remarkable bird as a Scottish species, I made a very careful summary of the records of its occurrence in North Britain, from the earliest published accounts of the bird until its final disappearance; and to this account, which is given in the "Birds of the West of Scotland,"* I may perhaps be permitted to refer.†

* Gray, "Birds of the West of Scotland, including the Outer Hebrides," 1871, pp. 441–453.

† I have the pleasure of inserting here the following letter which has been addressed to me since this paper was read. It narrates the capture of the last of the Auks in Scotland:—

"EDINBURGH, 17*th June* 1880.

"MY DEAR SIR,—I think you will be interested in knowing that when at St Kilda on the 14th of this month I found there was a man still living there who assisted at the capture of Fleming's Great Auk in 1821–22.

"Having shown a drawing of an Auk to the collected natives to see if they had any knowledge of it, they said they knew it used to be there long ago, but they had never seen it. Subsequently they told me the man was still there who *caught the last Great Auk.*

"I had him immediately brought to me. His name is Donald M'Queen, Sen. He is a very little man, and is also so much bent, that he does not now stand much higher than the Great Auk did. He said he was 73 years of age, but to all appearance he is considerably more.

"Donald disclaimed having been (as his neighbour reported) the person who actually caught the Auk. He informed me that he was one of four persons in a boat on the east side of the Island when they discovered the bird sitting on a low ledge of the cliff.

"Two of their number (then young men) were landed, one on either side of the bird, and at some distance from it. These two cautiously approached it, whilst he and another boy rowed the boat straight towards the Auk, which ultimately leaped down towards the sea, when one of the youths, having got directly under it, caught it in his arms. The old man with much animation went through the pantomime of grasping a supposed bird in his arms and holding it tightly to his breast.

"A partial error of the old St Kildian served to identify this Auk with Fleming's. He said the people who got it 'tied a string to its leg and killed it.'

Regarding its existence in other parts of the world there can be no reasonable doubt that the Great Auk had, as already mentioned, its headquarters on the rocky and almost inaccessible islets off the coast of Newfoundland. In these localities, if we may judge from the writings of the early navigators, it existed in very large communities, especially during the breeding season, and was taken in boatloads for sustenance to the ships' crews. We read, for example, in Captaine Richard Whitbourne's "Discourse and Discovery of Newfoundland," published in 1622, that "these Penguins" (the name by which the Great Auk was known on the coasts) "are as bigge as geese and fly not, for they have but a little short wing, and they multiply so infinitely upon a certaine island that men drive them from thence upon a boord into their boats by hundreds at a time, as if God had made the innocency of so poor a creature to become such an admirable instrument for the sustentation of man." Other writers speak of parties landing upon the rocks and knocking down with clubs hundreds of Auks, which they carried to their ships to be used as food. In short, we know that nearly all the French sailors of that and later periods trading between Havre de Grace or other French ports and Newfoundland, victualled their ships regularly with Great Auks taken in such places as the Funk or Penguin Islands, which are situated on the east coast; and that on leaving home they regulated their supply of provisions accordingly. It is stated in a letter written from Bristow to Mr Richard Hakluyt of the Middle Temple by Mr Anthonie Parkhurst, and dated the 15th of November 1578, that "the Frenchmen that fish neere the grand baie doe bring small store of flesh with them, but victuall themselves always with these birdes"; * and again, in "a report of the voyage and successe thereof, attempted in the yeere of our Lord 1583 by Sir Humfrey Gilbert, knight, &c., by Mr Edward Haies, gentleman, &c., it is stated that the French men barrell them vp with salt." † Such a fate for the poor Auks was foreshadowed in 1536, in which year Penguin Island, one of their haunts,

"When I told him they did not kill it, he said he might have forgotten what he had heard about it after it was taken away.

"Mr Mackenzie, the factor of the island, who was present, said Donald M'Queen was a trustworthy person, and that I might rely upon his telling the truth.—Yours faithfully, R. SCOT SKIRVING."

* Hakluyt, vol. iii. pp. 172, 173.

† *Ibid.* vol. iii. p. 191.

was visited by a Mr Hore "and divers other gentlemen," who recorded having seen a "great number of foules" which were "very good and nourishing meat"—a hint which appears not to have been lost sight of by subsequent navigators. "A person of the name of Hore, says Forster [Robert Hore], set sail in 1536 from London with two ships—the 'Trinity' and the 'Minion'—about the latter end of April. They arrived at Cape Briton, and from thence went to the north-eastward till they came to Penguin Island, an island situated on the southern coast of Newfoundland, and which was named thus after a kind of sea-fowl which the Spaniards and Portuguese called *Penguins* on account of their being so very fat, and which used to build their nests and to live in astonishing quantities on this little rock."* Four years later (1540) Jacques Cartier in his "Third Voyage" refers to the slaughter of these birds by himself and his crews, and speaks of loading his two vessels with dead Penguins in less than half an hour, as he might have done with stones, so that, not reckoning those that were eaten fresh he had in each vessel four or five tons of them put in salt.

No species of bird, especially one to which the power of flight had been denied, could long survive such wholesale destruction as these French sailors narrate; and as we have already seen that subsequent traders continued to make very serious inroads upon the haunts of the doomed Penguin, it need excite no surprise that by the time the attention of scientific writers was drawn to the bird, it had almost become a rare species. In a published form there is but little to narrate, between the time of Whitbourne's visit and the inquiries made by Audubon when preparing his work on the "Birds of America," in 1831. Writing in 1684, William Dampier † states that he had seen Penguins plentifully on the coast of Newfoundland; and in 1750 George Edwards, author of a meritorious work on the "Natural History of Birds," figured a Great Auk in vol. iii. pl. xlvii., which he states he procured from the master of a Newfoundland fishing-vessel who captured it with fish bait on the fishing banks about a hundred leagues off shore.

* History of the Voyages and Discoveries made in the North, by John Reinhold Forster, J.U.D., London, 1786, p. 290.

† New Voyage round the World, 3d ed., London, 1698.

Again, we find from a "Journal of Transactions and Events during a Residence of nearly Sixteen Years on the Coast of Labrador," by George Cartwright, that in 1785, the systematic destruction of Penguins, had led to the disappearance of these birds, in the breeding season at least, from all their known haunts around the coast of Newfoundland, with the exception of Funk Island:—

"1785. July, Tuesday 5.—A boat came in [to Fogo Harbour] from Funk Island laden with birds, chiefly Penguins. Funk Island is a small flat island rock, about 20 leagues east of the island of Fogo in the latitude of 50° N. Innumerable flocks of sea-fowl breed upon it every summer, which are of great service to the poor inhabitants of Fogo, who make voyages there to load with birds and eggs. When the water is smooth they make their shallop fast to the shore, lay their gang-boards from the gunwale of the boat to the rocks, and then drive as many Penguins on board as she will hold; for the wings of those birds being remarkably short they cannot fly. But it has been customary of late years for several crews of men to live all the summer on that Island for the sole purpose of killing birds for the sake of their feathers, [and] the destruction which they have made is incredible. If a stop is not soon put to that practice the whole breed will be diminished to almost nothing, particularly the Penguins, for this is now the only island they have left to breed upon; all others lying so near to the shores of Newfoundland they are continually robbed. The birds which the people bring from thence, they salt and eat, in lieu of salted pork." *

The same author states that the Red or Wild Indians of Newfoundland visited Funk Island every year.

In 1819 Anspach mentions that, at the time he wrote, the Penguin †

* Vol. iii. p. 55. At page 222 of the same volume, the author writes as follows—and the quotation may serve to show that if such islands as are alluded to were at one time inhabited by Great Auks, the birds may have had other enemies to contend with besides human invaders:—"All along the face of the east coast, and within the many capacious bays which indent it, are thousands of islands of various sizes, on which innumerable multitudes of Eider Ducks and other water-fowl breed. The very smallest are not without their inhabitants, if the spray of the sea does not fly entirely over them; and the larger ones have generally deer, foxes, and hares upon them. The former will swim out to them to get clear of the wolves which infest the continent; but the two latter go out upon the ice, and are left upon them when it breaks up in the spring."

† Pinwing is now the name in use among old residenters, at the various

had been extirpated from its old haunts;* and in 1838, Audubon, who has very little indeed to say regarding the species, and nothing whatever from personal observation, writes as follows:—"The only authentic account of the occurrence of this bird on our coast that I possess was obtained from Mr Henry Havell, brother of my engraver, who, when on his passage from New York to England, hooked a Great Auk on the Banks of Newfoundland in extremely boisterous weather. On being hauled on board it was left at liberty on the deck; it walked very awkwardly, often tumbling over, bit every one within reach of its powerful bill, and refused food of all kinds. After continuing several days on board it was restored to its proper element."† This, as I have already remarked, was probably the last time the Great Auk was seen in that part of the world. The same author also states that when he was in Labrador (no date is given) many of the fishermen had assured him that the "Penguin," as they named the bird, bred upon a low rocky island to the south-east of Newfoundland, and that great numbers of the young were destroyed for bait. Corroborative information had been given him by several individuals in Newfoundland.

From that time until the present day all our information regarding the bird is more or less traditional in its nature. In 1841, however, ornithologists and others interested in the fate of the species were startled by the announcement made by Peter Stuvitz, a Norwegian naturalist, that he had collected quantities of Penguins' bones on the Funk Islands, and had seen the ruins of the rude stone enclosures into which former visitors had driven the poor birds before being massacred. ‡ Many of these bones are now in the Museum of the University of Copenhagen, and were the first relics

fishing stations on the coast of Newfoundland, who still remember the bird and its odd figure.

* "There was formerly on this coast a species of birds of the diving genus, which from their inability to fly were always observed within the space between the land and the Great Bank, and were once so abundant as to have given their name to several islands on that coast, but they are now utterly extinct. They were known by the name of Penguins."—*History of Newfoundland*, by L. A. Anspach, 1819, p. 393.

† Orn. Biog., vol. iv., 1838, p. 316.

‡ The Zoology of Ancient Europe, by Alfred Newton, M.A., Cambridge, 1862

obtained as corroborative proof of the correctness of the various records by the French sailors whose annual visits and slaughter of the defenceless birds have been already referred to. Shortly before Stuvitz made this discovery, Mr J. B. Jukes, who was engaged in a geological survey of Newfoundland in 1839, thus refers to a group of islands nearer the mainland :—" Aug. 26. At dawn we were under weigh. We sailed along shore as far as Dead Man's Point where the sand beaches ended and a rocky shore began; and then, passing by some low rocks called the Penguin Islands, sailed through the islets called the Wadhams. There was a large island of ice aground off these islands. Penguins were formerly so abundant on these shores that their fat bodies have been used for fuel: they are, however, now all destroyed, and none have been seen for many years." * Writing in the same year in which Mr Jukes' book was published, viz., 1842, Sir Richard Bonnycastle has the following remarks :—" In winter many of the Arctic ice birds frequent the coast, but the large Auk, or Penguin (*Alca impennis*), which not fifty years ago was a sure sea-mark on the edge of and inside the banks, has totally disappeared, from the ruthless trade in its eggs and skin." †

Mention may here be made of a mummified specimen of the bird which was procured from Funk Island in 1863, and forwarded to Professor Newton; and also of three other specimens, preserved in a similar way, from the same locality, which were obtained in the following year. The first formed the subject of a communication to the Zoological Society of London by Mr Newton, and is referred to as, with one exception, the only approach to a complete skeleton existing in Europe; the others passed into the hands of Professor Agassiz, and the British Museum. All these specimens were in a fair state of preservation owing to the antiseptic property of the soil: they were found 3 or 4 feet below the surface, under a covering of ice, about 2 feet in thickness. ‡

* Excursions in and about Newfoundland during the years 1839 and 1840. London, 1842, vol. ii., pp. 115, 116.

† Newfoundland in 1842, by Sir Richard Henry Bonnycastle, Knt., Lieut.-Col. in the Corps of Royal Engineers, vol. i., p. 232.

‡ In "A Short American Tramp, in the fall of 1864" (Campbell), p. 115, the author writes, "About 40 miles outside lie the Funks. Here used to be great numbers of Geyer fogel. Their skeletons are now brought to St Johns with guano."

Following these discoveries chronologically, we now come to the visit of Mr John Milne to the Funk Islands in 1874, an account of which was contributed by that gentleman to the "Field" newspaper, but afterwards published in a separate form. In this paper, entitled "Relics of the Great Auk on Funk Island," Mr Milne has given a most interesting description of the Island and its feathered inhabitants; and as but few persons possess a copy of the pamphlet on account of its scarcity, no apology seems necessary for giving a short extract from the author's narrated experiences on this old dwelling-place of the Penguin.

"At the distance of half a mile the island looked not unlike a smooth-bottomed upturned saucer, slightly elongated into an ellipsoidal form towards its north-eastern extremity, from which end it sloped more gradually up from the sea than it did from its opposite end. As we drew near a few irregularities could be seen along its northern half, which afterwards we found to be heaps of large boulders. Immediately in front of us there was a small cliff, in a crevice of which, we understood, was the usual place of landing. To get ashore at this point—which, as a rule, is the most accessible on the island—is a matter of difficulty. First the boat is rowed alongside the cliff, on the face of which there is a ledge leading to higher ground. The next thing is to balance yourself upon one of the seats or thwarts of the boat whilst it rises, falls, and rolls upon the ever heaving swell. You now wait your chance until you think yourself sufficiently high for a spring. You make it, but it must be without hesitation, and you are landed on your perch. A short scramble, and you are upon the high ground, gazing down at your companions, expanding and contracting as they rise and fall upon the waves, balancing themselves like acrobats whilst waiting to follow your example. However, we were saved from this exhibition of agility by finding a comparatively smooth corner a little further to the north, where, under a shrieking and wailing cloud of birds, one by one we jumped ashore and clambered to a secure foothold."

* * * * * * *

"On the island are several remains of rough stone-work. These, in fact are said to have been used by the now extinct aborigines of Newfoundland, and also by sailors in later times as pens, into which

they drove the Garefowl there to wait until they should be required for use."

*   *   *   *   *   *   *

"Having a strong wish to secure some relics of this bird, and my time for their discovery being limited to less than an hour, it was with considerable excitement that I rushed from point to point and overturned the turf. At nearly every trial bones were found; but there was nothing that could be identified as ever having belonged to the bird, for which I searched. At the eleventh hour the tide turned, and in a small grassy hollow, between two huge boulders, on the lifting of the first sod I recognised an alcine beak. That rare element called luck was in operation. In less than half an hour specimens indicating the pre-existence of at least fifty of these birds were exhumed. The bones were found only from 1 foot to 2 feet below the surface, and in places even projected through the soil into the underground habitations of the Puffins. With the exception of one small tibia and two or three tips of long and thin beaks, probably those of the Tern, all the bones were those of the Great Auk."

*   *   *   *   *   *   *

"In several cases whilst exhuming the skeletons I noticed that the vertebræ followed each other successively, and were evidently in the same position which they occupied when in the live bird. This is in part confirmed by one curious case, where the rootlet of some plant has grown through the neural canal and expanded so at to fix the vertebræ in position. This, together with the fact that there remains no evidence of cuts or blows, leads to the supposition that these birds may have died peacefully. Nevertheless, it may be that they were the remains of some great slaughter, when the birds had been killed, parboiled, and despoiled only of their feathers, after which they were thrown in a heap such as the one I have just described."

Mr Milne also alludes to a considerable difference in size which he observed in several of the bones, but states that the only trace of the bird having been used as fuel was a single burnt fragment of a sacrum. It is, however, possible that with more time at his disposal he might have made further researches which would have thrown some light on traditional records bearing on this subject.

In the last published work on Newfoundland I find the following remarks:—

"The Penguin or Great Auk (*Alca impennis*, Linn.) about seventy years ago was very plentiful on Funk Island, but has now totally disappeared from the coast of Newfoundland. Incredible numbers of these birds were killed, their flesh being savoury food, and their feathers valuable. Heaps of them were burnt as fuel to warm the water to pick off the feathers, there being no wood on the island. The merchants of Bonavista at one time used to sell these birds to the poor people by the hundredweight instead of pork."*

On examining the two eggs which are now upon the table, it will be seen that the word "Egale," written upon each, points to a former French possessor; but if the writing means the name of a rock or islet, I have been unable to trace any such locality on the maps I have consulted, although these are somewhat minute in their indications of both along the entire coast-line of Newfoundland. Professor Newton informs me that on some of the eggs which he has examined the words "St Pierre" are plainly written, which probably signify that such specimens have come from some rocky islet in the neighbourhood of that island, if not from St Pierre itself. This locality is off the southern coast of the group called "Miquelon,"† and is not far distant from the Penguin Islands, visited by Hore and others in the sixteenth century. A glance at any well-prepared map of Newfoundland, including the south-eastern shores of Labrador,‡ will show that all round the coasts there are numerous bays, with rocks and islands bearing such names as Penguin Island, Penguin Isles, Bird Island, Bird Isles, Murr Island, Murr Rocks, Gull Island, Duck Island, Shag Rocks, Cormorant Rocks, Petrel Islands, &c., and we not only infer that all those named "Penguin" were so called on account of the obtrusive numbers of Great Auks frequenting them, but that many of the

* Newfoundland as it was, and as it is in 1877, by the Rev. Philip Tocque, M.A., London and Toronto, 1878.

† Lieutenant Chappell, an intelligent and observant cruiser, in his "Voyage to Newfoundland in H.M.S. 'Rosamond,'" refers to this group (1818), but makes no allusion whatever to the Penguin.

‡ A very good map has been published in the "History of Newfoundland," by the Rev. Charles Pedley (1863). The map accompanying Bonnycastle's "Newfoundland in 1842" may also be consulted with advantage.

others were likewise inhabited by these birds. Even within comparatively recent times Great Auks have been known to occur in some numbers on a rocky islet in Bonavista Bay. Dr William Anderson, late of Brigus and now of Heart's Content, writes to me that Mr Alfred Smith residing at Cupids, and other aged residenters, have informed him that they remember this haunt being partially occupied, and that quantities of the birds were, in olden times, used both as food and fuel. Another informant has stated to him that he remembers seeing, when a boy, Great Auks on the Funk Islands. In sailing past, the birds were pointed out to him as they sat upon the rocks, and the impression he had formed of their size and upright figure had never been effaced. Mr Smith at same time informed Dr Anderson that ten years ago at Manok, or Mannock Island, Labrador, he saw in the hands of some Indians what they spoke of as a young Pin-wing. The length of the bird was equal to that of his hand, and "half-way up the fore-arm." The Indians told him they had picked up the bird dead, but whether on ice, water, or strand he could not ascertain. Dr Anderson, however, whose letter is dated 28th September 1879, cautiously adds that, on further investigation he discovered that these Indians had at the same time a live Porcupine, among other things, for sale or barter, which showed they had been "in the curiosity line."

Another informant—Joseph Bartlett—stated to Dr Anderson that he had often heard his father, who died in 1871 at the age of 70, speak of the Pin-wing; and that crews occasionally got on the Funks, built enclosures, lit fires, and burnt the birds to death for pure mischief. Several other aged masters of fishing-vessels, who have been spoken to by Dr Anderson, recollect perfectly hearing their fathers refer to both birds and eggs which they had taken; and Mr Smith especially referred to the eggs being of "one pint capacity," and the feathers of the bird being of considerable sharpness, readily pricking the skin and causing festering. None of the aged people, however, examined by my correspondent, seem to be able to fix a precise date for the Penguin's disappearance from the Newfoundland habitats.

In alluding to the former existence of the Garefowl in Iceland, I may refer to an excellent paper on the subject contributed by Professor Newton of Cambridge to the "Ibis" for 1861, entitled

"Abstract of Mr Wolley's researches in Iceland respecting the Garefowl or Great Auk."* In this very able paper a graphic and circumstantial account is given of the capture and death of the two latest survivors of their species, which event took place on a rock called Eldey, or Fire Island, by the Icelanders, and by Danish sailors, Meel Sækken, or the Meal Sack, on the 6th June 1844. The chief actors in this memorable undertaking were three Icelanders named Jón Brandsson, Sigurðr Islefsson, and Ketil Ketilsson. Experiencing the greatest difficulty in landing upon the rock, the three men, as they clambered up, saw two Garefowls sitting among the numberless other rock-birds, and at once gave chase. "The Garefowls showed not the slightest disposition to repel the invaders, but immediately ran along under the high cliff, their heads erect, their little wings somewhat extended. They uttered no cry of alarm, and moved with their short steps about as quickly as a man could walk. Jón, with outstretched arms, drove one into a corner, where he soon had it fast. Sigurðr and Ketil pursued the second, and the former seized it close to the edge of the rock, here risen to a precipice some fathoms high, the water being directly below it. Ketil then returned to the sloping shelf whence the birds had started, and saw an egg lying on the lava slab which he knew to be a Garefowl's. He took it up, but finding it was broken, put it down again. Whether there was not also another egg is uncertain. All this took place in much less time than it takes to tell it. They hurried down again, for the wind was rising. The birds were strangled and cast into the boat." And so died the last of the Great Auks. †

The commander of this expedition, on reaching the shore with his ill-gotten booty, started at once for Reykjavik to take the birds to Carl Siemen, at whose instance the expedition had been undertaken; but on his way he seems to have met a knowing purchaser, to whom he sold them for about £9. Allusion is also made in Professor

* Ibis, vol. iii. 1861, pp. 391, 392.

† I have been kindly informed by Mr Wenley of this city, that in July of the present year he had, through the attention of Professor Steenstrup, an opportunity of seeing the remains of these two specimens in the University Museum at Copenhagen. They are simply anatomical preparations, consisting of the intestines and other internal organs—the muscles, bones, skins, and feathers not having been preserved.

Newton's paper to the disappearance of a range of rocky skerries in Iceland, the Geirfuglasker, which was engulphed by the sea in 1830 during a submarine volcanic disturbance, a catastrophe which contributed very materially to the birds' destruction. This range was a noted haunt of the Great Auk for centuries, and the eruption which overwhelmed it seems to have been a final blow towards the extinction of the species.

In connection with Iceland it may not be out of place to refer to the name given to the Great Auk by Niels Horrebow, whose work on Iceland appeared in 1752, viz., the "Geir, or Vulture." Whether this writer had traced any connection between the Iceland name *Geirfugl* and *Lammergeir*, or *geyer* (literally, "lamb vulture"), which is a connecting-link between the Eagle and Vulture, I am not prepared to say—the etymology of the name Garefowl being confessedly a difficult question. Professor Newton informs me that the obvious resemblance at first sight between *Geir* and the German *Geier* or *Geyer* (its older form) has struck several persons, but that he doubts if it is more than a coincidence. The following is Horrebow's account of the Garefowl which I have not seen quoted in any recent publication :—

"The Vulture Rocks, called also Bird Rocks, lie beyond Reikenes in the south district, about 6 or 8 leagues west of this place. On these cliffs and rocks are a great many Vultures, which, besides, harbour in other parts of the island. The inhabitants at a certain season go to these islands, though the expedition is very dangerous, to seek after the eggs of this bird, of which they bring home a cargo in a boat big enough for eight men to row. The danger and difficulty consist in getting ashore near these cliffs, which lie 6 or 8 leagues out at sea, where the water generally runs so high that if the boat be not carefully managed it runs the risk of being dashed to pieces against the rocks by the violence of the waves. Though there are not so many of these birds as of other sea-birds, yet they are not scarce. They are frequently seen; and those that go to take their eggs from them see enough of them. The eggs are very large, and about as big as Ostriches'." Horrebow also quotes the authority of Herrn Johann Anderson, who states that "the Geir, or Vulture, is not often seen in Iceland except on a few cliffs to the west; and that the Icelanders, naturally superstitious, have

a notion that when this bird appears it portends some extraordinary event. Of this he assures us being told that the year before the late King Frederick IV. died there appeared several, and that none had been seen before for many years." * It is worthy of note that in "the new and general map of the island" accompanying Horrebow's work, the Geirfuglasker and Eldey are there marked as "Vulture, or Birds Islands."

It may not be out of place here to refer to the fact of both French and English writers using the term Pingouin or Penguin, in speaking of the Razor Bill (*Alca torda*) as well as of the Great Auk. Thus, Buffon (Ois. vol. ix. p. 393) has given Le Grand Pingouin as the name of the Great Auk, while the Razor Bill is simply Le Pingouin. Temminck (Manuel, vol. ii. p. 937–939) also gives the name *Pingouin brachiptère* and *Pingouin macroptère;* the former for the Great Auk and the latter for the Razor Bill. MacGillivray (British Water-birds, vol. ii. p. 346) applies the name *Gurfel* to the Razor Bill, while Fleming (British Animals, 1828, p. 130) introduces as Welsh synonymes for the same bird, *Garfil* and *Gwalch y Penwaig.* The name Penguin had apparently at one time been applied in this country to the Razor Bill in popular works, as I find from a map of the Western Isles, published in Edinburgh in 1823, in which it is stated that "the south-west coast of Bernera and Mingulay are remarkably bold precipices rising perpendicularly from the sea in lofty cliffs of gneiss which are frequented in summer by innumerable flocks of Puffins, *Razor-bill Penguins* and Kittywakes. These birds disappear early in autumn with their young."

For the last forty years, if not for a longer period, the money value attached to the eggs and skins of the Great Auk has contributed in a very material degree to the destruction of the species. Caterers for collections, public and private, caused a demand, to supply which organised parties visited the bird's haunts even at the peril of their lives, and effectually exterminated the bird. The very last expedition, as we have seen, resulted in but two birds being captured, and it has a most melancholy interest when we reflect that from that time till now the Great Auk has been a thing of the past. Judging from published records it would seem that there are about seventy skins and about as many eggs of this bird

* Natural History of Iceland, &c., by N. Horrebow, folio, London, 1758.

in existence in European and American collections. That both are of great value no one can doubt. Indifferent skins may be worth from eighty to one hundred guineas, and indifferent examples of the egg more than half that sum. A really fine specimen of the egg, blown at the side, if there be one in existence, must be valued at its weight not in gold, but in bank notes. No wonder, then, that unprincipled persons have been known to imitate both egg and skin —the counterfeit egg especially being a consummate work of art.

Although much has been written of late years about the Garefowl, a complete monograph on this extinct bird is still a desideratum in scientific literature; and ornithologists have for some time been in expectation of seeing such a work produced. The name of Professor Newton of Cambridge University has been mentioned, and naturalists of all nations allow that the task of writing the Great Auk's memorable history could not be committed to abler hands.

5. On a New Telephone Receiver. By Professor Chrystal.

The experiment which forms the subject of this communication was originally devised as an illustration of the explanation of all kinds of microphone receivers, suggested by the beautiful experiments of Mr Blyth, on loose contacts. My idea was to replace Mr Blyth's heated point of metal by a continuous portion of the circuit which should act in the same manner.

It was obvious, for two reasons, that this part must be of small diameter; 1*st*, in order that the resistance, per unit of length, might be great enough to make the variation of the heating sufficient to cause sensible longitudinal extension; 2*d*, in order that the section might be small enough to allow sensible cooling in, say the $\frac{1}{300}$th of a second. I had reason beforehand to believe that the second of these conditions could be fulfilled in practice; because, I found in my experiments on Ohm's Law (Brit. Ass. Rep. 1876, p. 58, *et seq.*) that, when currents of two different strengths alternated, even with great rapidity (60 times per second) in a fine wire, the resistance was sensibly higher during the passage of the stronger current.

My first experiment was tried with Mr Blyth's apparatus. A fine platinum-iridium (10 p. c. Ir. Res. ·7 Ohm per Cm.) wire, about

5 Cm. long, was soldered to a tolerably thick copper wire, which served as a terminal; the other end was fixed securely to the copper spring attached to the mica-diaphragm of the ear-piece. The whole was then put in line with four Bunsen's cells, and a microphone attached to a violin. The experiment succeeded at once. The music was perfectly audible close to the diaphragm, and a tune was reproduced quite distinctly.

For convenience in experimenting with different wires, I constructed the apparatus which I now exhibit to the Society. It consists of a fine palladium-silver wire (4 Ag. 1 Pd. Res. ·52 Ohm per Cm.), 8 Cm. long, soldered to two copper terminals, which are well amalgamated, and lie in the mercury of two cups forming the line terminals. One terminal is hooked to the membrane of a toy drum,* the other end of which is removed; and the other terminal is attached to a string, to which is hung a scale pan, with small weights for producing the requisite tension.

With this apparatus, I can reproduce the music of the violin in the far room, so that all present can hear it. The roughness which mars the effect, is simply due to vibrations of the microphone, which happen to be in unison, now and then, with the note of the violin.

I have satisfied myself that the action of this instrument is not due to loose contacts, or to the earth's magnetism. I believe it to be due to the variations in the heating of the wire, which follow the variations of the current strength caused by the microphone. The tension of the wire does not seem to be material, farther than that there must be a certain tension before the effect is produced; for a wire absolutely loose, gives little or no effect. A thick wire of platinum, with the four cells, did not act until it was made very short; and a wire of copper, 4½ Cm. long and about ·01 Cm. in diameter, would scarcely act at all. The only apparent exception that I found was iron. I found that I could get a tolerable result with an iron wire 4 Cm. long, and thicker than the copper wire last mentioned. A fine steel wire hairspring acted very well, but not so well as the long palladium silver wire. I also tried other metals, but none surpassed the platinum and palladium-silver wires.

So far as I have been able to go with a very fine wire, the effect

* For reproducing articulate speech, a small mica-diaphragm like those used by Edison, Blyth, and others, is best.

increases with the length. There must, of course, be a limit to increase in this way; but, unfortunately, the stock of fine wire obtainable in Edinburgh is neither very varied nor very extensive; so that I could not examine this point farther.

With the same wire, the effect increased with the current strength. The notes came out best when the current just heated the fine wire to a very dull red. It was beautiful then to see the wire burst into a bright glow when reproducing a prolonged note, especially a high one. This glow is sometimes so strong that the wire softens and breaks under the tension. When the wire is shielded from air currents, the glow can be seen to follow the swell of the music, and, with a wire 10 or 12 Cm. long, the motion of the end could be seen quite distinctly keeping time with the swell of the music.

Heating the fine wire *externally* by means of a lamp increases the effect slightly, and cooling with water or a blast of air seemed to produce the opposite effect, but only slightly in the case of very thin wires.

[*Added May* 18*th*.—Since the above experiments were made, my attention has been directed to a paper by Dr Ferguson (Proc. Roy. Soc. Edin., 1877–78, p. 628), in which he anticipates to a certain extent, the main experiment above described. It is true that he has not applied his apparatus to the transmission of music or articulate speech as I have done, but he makes the practically very important step of attaching a mechanical telephone to the wire conveying a varying current, and thereby renders the observation of the sounds of De La Rive both easy and certain. He has also given the very important result that these sounds may be caused by currents of very small total heating effect, such as induction currents. This I have since verified in certain cases.

Dr Ferguson is of opinion that these sounds are not due to heating effects but to some other *molecular* cause, which he does not very clearly define. Except in the case of iron, I see no reason as yet for so explaining them. It must be remembered that it is not the *whole* heating effect that is the question, but its *variation* in a very short time, say the $\frac{1}{500}$th of a second, so that there may be no inconsistency in explaining the ticks in Mr Ferguson's experiments and the music in my own as due to the same cause.

The exceptional behaviour of iron in the case of thick wires (in very thin wires its superiority is doubtful) is not surprising. Professor Tait has suggested a farther anomaly, viz., that at a very high temperature iron may be incapable of producing these sounds altogether.

I am at present in possession of some very interesting results bearing on this point, which I propose to lay before the Society at some early meeting.

It happens, very curiously, that Mr Preece, apparently about the same time as myself, was led to devise an instrument practically identical with the one I exhibited to the Society. An account of it appeared in "Nature," vol. xxii. p. 138; being an abstract of the Proceedings of the Royal Society of London, on May 27th.]

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Mr W. F. King, Professor MacGregor, Halifax, N.S., Mr Patrick Geddes, and Dr W. Robert Smith.

Monday, 21*st June* 1880.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. On the Differential Telephone, and on the application of the Telephone generally to Electrical Measurement. By Professor Chrystal.

The plans and calculations in this paper are now more than two years old, but the author has only lately, by the kindness of Professor Tait, found opportunity to carry them out in practice.

A discussion is given of the different methods of applying the telephone to accurate measurement, and mention is made of the points which the author thinks have been missed by most of those who have worked in this way hitherto.

A common principle runs through all telephonic null methods, viz., that the balance may be dependent on the frequency of the interrupted current or may be independent of it. In the latter case

there are in general more than one condition of balance, and in the former case, although one will very often suffice, two may occasionally be necessary. In the former class of cases we get relations between electric quantities of the same dimension, in the latter relations between quantities of different dimensions, *e.g.*, between coefficients of induction and resistances: so that when the frequency is known we can find a coefficient of induction in terms of a resistance, and so on.

A new instrument is described called the differential telephone. It is an ordinary telephone only wound double like a differential galvanometer. The peculiar difficulties attending the construction of an instrument of this kind which will give no sound when the same current passes round its two parallel circuits in opposite directions are explained, and the means of overcoming them pointed out.

The method of using the instrument is explained. A multiple circuit of two branches A and B is inserted in a circuit containing a battery and an interruptor. A and B each contain one coil of the differential telephone, so that the currents pass in opposite direction round it. A and B have self-induction coefficients, M and N, which can be varied at will by altering the configuration of certain coils in the two circuits. If the resistances of A and B be Q and R, then the conditions of equilibrium are shown to be $M = N$, and $Q = R$. There cannot be silence if either of these is unfulfilled, and if both are fulfilled there is silence for all frequencies of the interruptor.

It is pointed out that the instrument, and in fact the telephone generally, is better suited for measuring coefficients of induction than for measuring resistance.

A practical method for procuring a graduated scale of coefficients of induction is then explained.

The mathematical theory of the disturbance of the balance in the differential telephone by two independent circuits E and F neighbouring to A and B is given. If S and T be the resistances, G and H the coefficients of self-induction, and I and J the coefficients of mutual induction with A and B of E and F respectively, then the following four conditions

$$Q = R \qquad SJ^2 = TI^2$$
$$M = N \qquad GJ^2 = HI^2$$

must be satisfied in order that there may be silence for all frequencies of the interruptor.

It is also possible to obtain silence *for a given frequency* by satisfying two conditions, which are given.

The bearings of this theory on practice are pointed out, and the reason explained why the induction balance as arranged by Hughes, does not give results than can be interpreted satisfactorily.

The mathematical theory of the measurement of capacities is then given. If the armatures of two condensers of capacities X and Y be attached, by wires whose resistances may be neglected, to the circuits A and B, so as to include between them all the self-induction of the circuits except that of the telephone coils, it is shown that there cannot be silence for all frequencies unless

$$Q = R, \qquad M = N, \qquad X = Y.$$

Another method is described for finding capacities in terms of resistances. In the circuit A of the differential telephone is inserted a multiple arc, in one branch of which is a condenser of capacity X, the resistance of this branch is Q''. In the other branch there is resistance Q' and self-induction M'. The resistance and self-induction of the rest of A are Q and M, and the resistance and self-induction of B, R and N. The conditions for silence for all frequencies is

$$M = N, \qquad Q' = Q'' = R - Q, \text{ and } \frac{M'}{Q'} = Q'X.$$

This last of these conditions means that the time constants of the coil (M', Q') and the condenser (X, Q') shall be equal. When this is the case, the multiple arc behaves like a resistance Q', having neither induction nor capacity.

It is proposed to apply the differential telephone to the measurement of coefficients of induction, and to the comparison of capacities and their evaluation in absolute measure. It is also expected to prove useful in measuring specific inductive capacity, in investigating the properties of electrolytes, and in examining the internal resistance and polarisation of batteries in action. It is possible that the method last described may afford an improved determination of the ratio of the electrostatic to the electro-magnetic unit.

The rest of the paper is occupied with a discussion of the use of

the ordinary telephone in connection with Wheatstone's bridge. The mathematical theory of various cases likely to prove useful is examined, and their application to the comparison and evaluation in absolute measure of electrical quantities is discussed.

Several curious experiments with the differential telephone were shown to the Society. In particular it was shown that when an interrupted current was passed through one coil of the telephone so that the sound could be heard all over the room, by merely connecting up the parallel coil through a small resistance, the sound was so much deadened as scarcely to be audible at a distance. The deadening effect was shown to be much less when the resistance through which the idle coil was connected had considerable self-induction Grant's experiment ("Phil. Mag.," May 1880, p. 352) was also shown. The electrical theory of these experiments is given in the above paper.

[*Added, August* 19.]

Since the above abstract was written I have carried out the practical application of the above theory much farther. An instrument for giving a variable self-induction with constant resistance has been constructed, and promises to give very good results. I hope to lay a description of it before the Society next winter.

By the kindness of Sir William Thomson and Professor Fleeming Jenkin, my stock of available resistance coils and capacity standards has lately been much increased; and I have been able to satisfy myself as to the thorough practicability of all the above methods of electrical measurement. I reserve details in the meantime; but may mention that the differential telephone shows differences as to self-induction between the so-called induction-less resistance coils. I have measured this difference in the case of two 1000 coils in one of Elliot's boxes, and find it about what might be expected from theory (see Maxwell, "Electricity and Magnetism," vol. ii. p. 291). I believe, from preliminary experiments on capacity measurements with this instrument, that it will be possible by means of it to compare capacities of the order of a microfarad to the $\frac{1}{100000}$ part (provided, of course, that capacity turns out to be definite to that degree of accuracy).

Since I have had the use of standards of capacity I have been

able to add to the general experiments with the differential and ordinary telephone above described. In particular, I have repeated the experiments of Grant in several striking forms, and made a variety of others of a similar nature. I have gone into the mathematical theory of these results, and, I think, succeeded in explaining the curious changes in the quality and intensity of the sounds observed. Among the results, I should desire particularly to draw attention to the theory of the striking alterations produced by condensers in the *pitch*, or, more correctly speaking, *quality* of telephone sounds.

2. On the Determination of the Specific Heat of Saline Solutions. By Thomas Gray, B.Sc., Demonstrator in Physics, and Instructor in Telegraphy, Imperial College of Engineering, Tokio, Japan. Communicated by Professor Tait.

The object of the present paper is to describe the results obtained and the mode of experimenting adopted in some determinations which I have made of the specific heats of solutions of salts. These experiments form part of a series which I am at present carrying out on the physical changes produced when salts are dissolved in different amounts of their solvents. From such an investigation I believe much information may be gained regarding the nature of solution.

The method of experimenting adopted in the experiments described below was that of mixtures; but as regards the mode in which the exact amount of heat added to the solution was measured, it differed from any process with which I am acquainted. This peculiarity consisted in using as heater a thin glass bottle of about 50 cubic centimetres capacity, and furnished with a long glass neck, just wide enough to allow an ordinary mercury-in-glass thermometer to pass through. This bottle was nearly filled with mercury, in which was immersed the bulb of a sensitive thermometer, and thus the temperature of the mercury in the bottle could be read off at any instant. The graduation of this thermometer was to fifths of a degree centigrade, and had been compared with the Kew standards. The distance between two consecutive divisions of its scale was about one millimetre.

The liquid, whose specific heat was to be determined, was placed in a thin glass beaker of about 350 cubic centimetres capacity, which in its turn was contained within a thick porcelain vessel of about two centimetres greater diameter. The two vessels were separated by a packing of cotton wool, which prevented in great measure loss of heat, and at the same time permitted the inside beaker to be removed and replaced with facility. To measure the temperature of the solution a very sensitive mercury-in-glass thermometer was employed. This thermometer, which had also been compared with the Kew standards, was graduated to tenths of a degree centigrade, and the distance between two successive divisions was about one millimetre.

It is evident from what has been stated above that a rise of temperature of one degree could be determined within two per cent. of its true amount, and therefore a rise of temperature of four or five degrees could be measured with great accuracy. There are other causes of inaccuracy, however, than incorrect reading of the temperature, of which the most important is perhaps the variation of temperature in the course of the experiment. All these causes of error were carefully allowed for, and in most cases three experiments made for each density of solution, the arithmetical mean of the results of which was taken as the true specific heat for the solution of the density in question.

The heater was arranged to have about one-tenth of the thermal capacity of the liquid, so that the temperature of the liquid experimented on should not be raised much above that of the atmosphere, and consequently only a small amount of heat be lost by radiation. The method of experimenting was as follows:—

In the first place the thermal capacity of the glass beaker was determined. This was done by filling it to about half the required height with water at the temperature of the atmosphere, and at the same time a similar vessel was filled with water about 10° above atmospheric temperature. The temperature and rate of cooling of this water were accurately determined, and then the temperature of the water in the beaker. The two quantities of water were then mixed, and the temperature read at the end of two minutes and again at the end of four minutes. The difference between these two readings added to the first reading gave the temperature of the

mixture. The differences between this temperature and each of the other two temperatures, viz., the high temperature corrected for cooling, and the low temperature, gave the fall and rise of temperature respectively. Calling now w the weight of water in the beaker, w_1 the weight of water added, t the rise of temperature, t_1 the fall of temperature, and c the thermal capacity of the beaker, we have

$$c = \frac{w_1 t_1 - wt}{t} \quad . \quad . \quad . \quad . \quad (1)$$

The mean of a set of five experiments made to determine c gave almost exactly 12 as its value.*

The thermal capacity of the heater was next determined. This was done by filling the vessel with the same volume of water as I intended to use of the solutions, and finding the rise of temperature produced in the water by nearly the same change of temperature in the heater as was to be used in the experiments. Putting w' for the weight of water, t' for the rise of temperature, t_1' for the fall of temperature of the heater, and c' for its thermal capacity, we get

$$c' t_1' = (w' + c) t'$$

or

$$c' = (w' + c)\, t_1' \quad . \quad . \quad . \quad . \quad (2)$$

It is manifestly of great importance that the value of c' should be known with accuracy, and accordingly the experiments for determining it were made with the utmost care. As the following results of a series of experiments do not vary by one per cent. from the mean result, we may conclude that the mean result is true to a fraction of one per cent.

Number of Experiment.	Capacity of Heater.
1	22·521
2	22·402
3	22·457
4	22·501
5	22·373
Mean result, . .	22·451

If now W be the weight of an equal volume of the solution, and

* The mass of this vessel was 70 grammes.

τ its rise of temperature corresponding to a fall τ_1 of the temperature of the heater, then

$$c'\tau_1 = (sW + c)\tau \quad . \quad . \quad . \quad . \qquad (3)$$

where s is the specific heat of the solution. Eliminating c' between this equation and (2) we get

$$\frac{(w+c)t'\tau_1}{t_1'} = (sW + c)\tau .$$

If we put $\tau_1 = t_1'$ the last equation becomes

$$(w+c)t' = (sW + c)\tau$$

or

$$s = \frac{(w)t'}{W\tau} + c\frac{t' - \tau}{W} .$$

Hence if, as the experiments prove to be the case, the specific heat per unit volume of the solution do not differ very much from unity, t' will be nearly equal to τ, and the value of s will not be materially affected by a small error in the determination of c.

The following is a description of the mode of performing an experiment. The volume of the solution is first roughly measured in a graduated flask, the liquid is then poured into the beaker and weighed. The beaker is then placed in the pad of cotton wool, and the thermometer put into position within it. The heater is placed on a hot plate and heated to about 95° C.; it is then lifted off and shaken up to bring the whole of the mercury to one temperature, and placed in the solution, the temperatures of the solution and of the mercury in the heater being carefully read just before the heater is immersed. Thus all uncertainty as to what the temperature of the heater was when it was placed in the liquid was avoided. The temperature of the solution was noted four or five minutes after the heater was placed in it, the liquid having first been stirred to equalise its temperature. After this interval of time there was no appreciable difference between the temperatures of the heater and liquid. Another reading of the temperature of the solution was made after the lapse of an equal interval of time, and the difference of the two readings added to the first to allow for cooling.

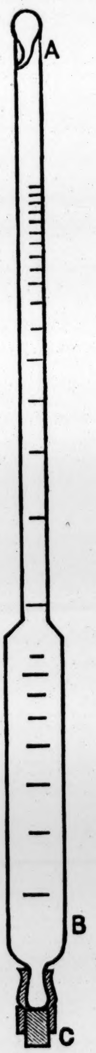
The following table gives the results of experiments on several solutions :—

Name of Solution.	Density.	Sp. Heat.	Mean S.H.	S.H. per unit volume.
Zinc sulphate in water.	1·327	·7453 ·7604 ·7520	·7526	·9987
	1·258	·7723 ·7799 ·7742	·7755	·9756
	1·161	·8586 ·8627	·8606	·9992
	1·075	·9210 ·9467 ·9014	·9230	·9922
Copper sulphate in water.	1·184	·8295 ·8361 ·8405	·8354	·9891
	1·142	·8660 ·8839 ·8864	·8788	1·0036
	1·109	·8883 ·9105 ·9007	·8998	·9979
	1·0871	·9051 ·9222 ·9306	·9193	·9994
Iron sulphate in water.	1·1523	·8450 ·8460 ·8495	·8468	·9758
	1·146	·8792 ·8769 ·8882	·8814	1·0101
Sodium chloride in water.	1·185	·8290 ·8438 ·8441	·8390	·9942
Sodium carbonate in water.	1·080	·9368 ·9248	·9308	1·0053
	1·0893	·9253 ·9230 ·9184	·9222	1·0046
Potassium bichromate in water.	1·0577	·9417 ·9467 ·9538	·9474	1·0021
Lead acetate in water.	1·216	·8217 ·8422 ·8318	·8319	1·0116
Lead nitrate in water.	1·1334	·8704 ·8976 ·8757	·8816	·9992

It is interesting to note the nearness in every case of the value of the specific heat per unit volume to unity. I am continuing my experiments, and hope later to be able to state the results of a more extended series of observations.

3. On a "Navigational" Sounding Machine. By J. Y. Buchanan.

The sounding machine of which the annexed figure is a representation, is intended for use at considerable depths while the ship is proceeding on her course. It is therefore necessarily capable of indicating the depths independently of the length of sounding line or wire used. It possesses the further great practical advantage, that when it has served its purpose once it is immediately available for another sounding.



It indicates primarily the extent to which a given volume of air at atmospheric pressure has been condensed by the column of water to which it has been subjected. As the law of the compression of air is known, the depth reached by the instrument is at once deduced.

The instrument consists of a tube A B, which may either be of uniform diameter, or made up of lengths of different diameters; in the figure the instrument is represented as made of two sizes of tube. This answers all practical requirements. The lower end B is contracted into a nozzle so as to receive a piece of india-rubber tube which can be plugged with a glass rod. At the other end A, a piece of tube drawn out to a moderately fine point is inserted, the point being bent slightly round, and fused hermetically into the end of the tube. The end A is thus closed by a sort of crooked funnel, which on the upper side is contracted to an orifice, at least as small as that of the end of the tube, projecting inwards. The object of this is to sift the water and allow no solid particles to enter which will not pass through the lower orifice of the funnel. The instrument is calibrated either by weighing or by measuring the water which it can

hold. As the volume assumed by the air is inversely as the pressure, it can at once be graduated into fathoms or other units of depth.

For use it is enclosed in a brass tube, and attached to the sounding line and allowed to sink. As it sinks the air is compressed, and its place taken by water which enters through the funnel, and being delivered in a fine stream against the walls of the tube runs down and collects at the lower end. On bringing the instrument to the surface again the water cannot get out, but the compressed air occupying the upper portion of the tube gradually expands through the orifice, which it thus completely occupies and prevents the entrance of water. Arrived at the surface, the depth is read off directly if the instrument is divided into units of depth, or if its scale is arbitrary the depth is found from a table constructed according to the results of calibration. When this has been done the plug C is removed, the water runs out, the plug is replaced, and the instrument is ready for use.

Last summer I had frequent occasion to test the accuracy of one of those instruments in the deep waters of Loch Fyne, and found it most satisfactory. The instrument which I used had an arbitrary scale of equal lengths, and had been very carefully calibrated by weight. From the results of the calibration I constructed a table of depths corresponding to the graduation, on the assumption that it was inversely proportional to the volume assumed by the air, and neglecting any effect of this pressure in causing increased absorption.

The following results obtained on 13th June 1879, while anchored in 87 fathoms off Garrock Head in Frith of Clyde, will show the

Depth (fathoms).	20	40	80
Found	90·5	182·0	243·0
	90·0	183·0	245·0
Mean, . . .	90·25	182·5	244·0
Calculated, ...	89·0	182·6	243·0
Difference, ...	0·25	—0·1	1·0

close agreement between the observed and the calculated depth. The instrument was sent down twice to 20, 40, and 80 fathoms, and

the reading noted. The calculated reading which it ought to have shown on the above assumption is put down, and it will be seen how closely the two agree.

This instrument is especially adapted for surveying-work where lines of sounding in comparatively deep water have to be run for considerable distances, as in the English or Irish Channels, or the North Sea. The sounding line or wire is arranged to pay over the stern, and arrangements are made for promptly heaving it in on bottom being reached. The engines are kept going at a convenient and uniform rate, and the revolution counteracted; it is then easy with good organisation to take soundings every hundred, two hundred, five hundred, or thousand revolutions. In the want of a revolution-counter, which, however, ought to be fitted to every marine engine, equally good results can be obtained if the engineer pays attention to keep his engine going uniformly, checking its rate at frequent intervals by counting the revolutions in a minute, by making the soundings at regular intervals of time. The number of revolutions made by the engines in a vessel of known capabilities is a very accurate means of ascertaining the distance run, and if this method were adopted the deep sounding of a survey could be worked off with great expedition and accuracy.

There is one property of this sounding-machine which must not be lost sight of, namely, that it registers the sum total of the increments of pressure. If it is to give the depth correctly it must both descend and ascend without interruption, and in the work for which it is designed this condition is always fulfilled. Suppose, for instance, it be sunk to 20 fathoms and be then drawn up to 10 fathoms, the corresponding quantity of air will be eliminated. If it is now again sunk to 20 fathoms, the place of the air which left while it was rising from 20 to 10 fathoms, will be taken by water, and if it now be brought to the surface it will of course register a depth greater than 20 fathoms.

It follows from the principle of this instrument that the value of its graduation scale will vary to a certain extent with the barometric pressure. For purposes of navigation the error so caused is negligeable, but if used for surveying purposes a correction must be applied.

The instrument is graduated for a barometric pressure of 30 inches.

As an inch of mercury exercises a pressure equal to $13\frac{1}{2}$ inches of water, we have for the corrected depth

$$D' = D\ \{1 - 0{\cdot}03\ (30 - H)\}$$

where D is the depth read off from the scale, and H is the barometric height. D′ the corrected depth is given in fathoms.

4. On the Compressibility of Glass. By J. Y. Buchanan.

(*Abstract.*)

The experiments related in this paper were undertaken with a view to determine, by actual observation, the effect produced on solids by hydraulic pressure. The instrument used consists of a hydraulic pump, which communicates with a steel receiver capable of holding instruments of considerable size, and also with a second receiver of peculiar form. This receiver consists essentially of a steel tube terminated at each end by thick glass tubes fitted tightly. It is tapped at the centre with two holes, the one to establish connection with the pump and the other to admit a pressure-gauge or manometer. The steel tube may be of any length, being limited only by the extent of laboratory accommodation at disposal. The tube which I am using at present has a length of a little over six feet and an internal diameter of about three-tenths of an inch. The solid to be experimented on must be in the form of rod or wire, and must, at the ends, at least, be sufficiently small to be able to enter the terminal glass tubes, which have a bore of 0·08 inch, and an external diameter of 0·42 inch. The length of the rod or wire is such that, when it rests in the steel tube, its ends are visible in the glass terminations.

The experiment is conducted as follows:—A microscope with micrometic eyepiece is brought to bear on each end of the rod or wire. These microscopes stand on substantial platforms, altogether independent of the hydraulic apparatus. The pressure is now raised to the desired height, as indicated by the manometer, and the ends of the rod are observed and their position with reference to the micrometer noted. The pressure is then carefully relieved, and a displacement of both ends is seen to take place and its amplitude noted. The sum of the displacements of the ends, regard being had to their

signs, gives the absolute expansion, in the direction of its length, of the glass rod, when the pressure at its surface is reduced by the observed amount, and consequently also of the compression when the process is reversed. As, in the case of non-crystalline bodies like glass, there is no reason why a given pressure should produce a greater effect in one direction than in another, we may, without sensible error, put the cubical compression at three times the linear contraction for the same pressure.

The rod experimented on was made of lead glass, drawn by Messrs Ford of Edinburgh, and was 75·05 inches long. The temperature of the water in the hydraulic machine varied from 12·5° to 13·5° C. The pressure varied from 1 to 240 atmospheres. Ninety-one separate observations were made, and the general result is, that the linear compressibility of the glass under experiment is 0·96, and its cubic compressibility 2·92 per million per atmosphere.

5. Suggestions on the Art of Signalling. By Alexander Macfarlane, M.A., D.Sc., F.R.S.E.

(*Abstract.*)

After considering the analogy which exists between the arts of writing and of signalling, the author proceeded to discuss what alphabet is the most suitable where the physical agent is not electricity. If we choose for elementary signals two qualities of the agent of communication A and B, which can be produced independently of one another, then the agent can be put into four states, viz., 1st, having the quality A, but not the quality B; 2d, having the quality B, but not the quality A; 3d, having both the qualities A and B; 4th, having neither of the qualities A and B. One of these states is required to separate letter from letter, and word from word; the fourth state where the agent is undifferentiated, is the one naturally adapted for the purpose. From the remaining three states we can get 3 permutations of one signal, 9 permutations of two signals, 27 permutations of three signals. Without going to a higher permutation than that of three signals, we get 39 symbols,*

* Three of these would probably require to be omitted as repeating the same signal three times.

which are sufficient for the numerals and all the letters contained in the Morse alphabet, less by one. These symbols we suppose assigned to the letters according to their frequency of occurrence as given in that alphabet.

In the case of the Morse system we have only three states of the agent; the third of the above states is not, or cannot be, made use of. As one is required for the purpose of spacing, only two are left to form symbols. To form equivalents for the 39 symbols spoken of above, it requires 2 permutations of one signal, 4 permutations of two signals, 8 permutations of three, 15 of four, and 10 of five. Thus in the former case 102 signals are required to form the alphabet, in the latter 144.

If the elementary signals of the Morse system are made to depend on a difference in quantity, then the above qualitative system possesses other two advantages. Its signals, as they differ in quality, can each be made to occupy the minimum time necessary for a signal to be observed, whereas in the other case the longer signal occupies thrice the time of the shorter; secondly, elementary signals differing in quantity require, though belonging to the same letter, to be separated by an interval, whereas those differing in quality do not.

By assuming that each of the qualitative signals can be sent in the same period of time as the short signals, also that the time required in the Morse quantitative system for the space between the elements of a letter is equal to that period, and the time required for a space between the letters to thrice that period, I have been able to calculate the relative times required by the two systems to signal the words London, Edinburgh, Dublin. The respective ratios are 2·8, 2·6, and 2·9. We may therefore conclude, assuming that the other advantages and disadvantages neutralise one another, that a message can be signalled 2·6 times as quick by the qualitative alphabet described as by the Morse (quantitative) alphabet.

The four-state alphabet would allow one elementary sign to be invariably associated with the right hand, and the other elementary sign with the left hand, and the compound elementary sign with both together. The effect of this would be that a person who had learned to signal by means of any one agent, would have almost equal facility in signalling with any other.

The author then proceeded to offer some suggestions as to how this alphabet could be applied in the respective cases of signalling by means of the heliograph, the light of a lighthouse, steam-whistles, flags, and touch; and advocated the opinion first brought forward by Dr J. A. Russell in a paper read before the Royal Scottish Society of Arts in 1875, that signalling should be taught in the primary schools.

6. Note on the Wire Microphone. By R. M. Ferguson, Ph.D.

At our last meeting Professor Chrystal showed us that a fine platinum wire attached to a stretched disc of skin could act as an electric telephone receiver for the sounds of a violin. The wire was included in a galvanic circuit, and the variations of current were made by a microphone attached to the violin. The account he gave of this interesting experiment was that the receiving wire became extended by the heat of the current either as it was established or suddenly increased by the microphone, and correspondingly shortened on the current ceasing. These extensions and contractions were rendered audible by the disc. A similar demonstration with a like commentary was made by Mr Preece to the Royal Society of London, an account of which was published in "Nature" (June 10). Mr Preece got his wires to speak. At the first May meeting of this Society in 1878 I discussed the subject of the sounds emitted by fine wires, giving passage to intermittent currents. I found that the ordinary thread telephone gave us an easy means of hearing these sounds in non-magnetic metals. De la Rive had heard them in 1845, but since his time no one had been able to hear them, and they were almost looked on as apocryphal. I attached the thread of the skin or paper telephone transversely to the sounding wire, and not directly, as Professor Chrystal has done, for the simple reason that I found that the transverse method gave equally good results with very much less trouble. The cause in both cases seemed to me the same, viz., an internal molecular click which marked the setting in and stoppage of the current. In the kindly reference that Professor Chrystal made to my communication he considered it strange that his simple explanation should have been overlooked, that the sounds should be set down as having conditions the same as those of heat and yet the

simplest and most certain effect of heat passed over. I must confess that the communication deserved that criticism, for the possibility of longitudinal extensions and contractions is not once referred to. At the same time I may say that I thought then, as I do now, that such extensions and contractions are not the cause of these sounds, and the object of this note is to give the ground for such a belief. I may shortly recapitulate why I thought so then.

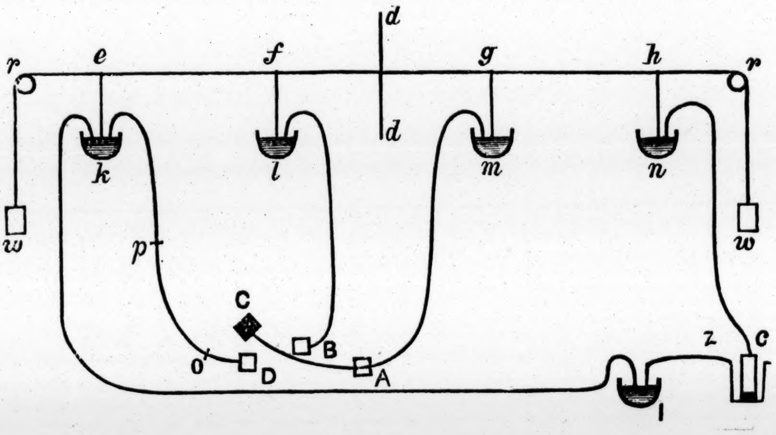
In the first place I was satisfied with the account given by De la Rive. He looked upon the sounds as a magnetic phenomenon. The wire became somehow magnetised and demagnetised by the beginning and end of the current, and the molecules of the wire, in taking and losing the magnetic set, hit against each other and emitted the sounds in question. He thought that such was the case from the exceptional position that iron occupied among the metals he worked with, and from the exactly similar action of wires within a magnetising spiral through which an intermittent current was sent, and those giving direct passage to the same current. The Bell telephone gave me an additional confirmation of this view. Any one who listens to the sounds emitted by discs of different metals when the telephone is excited by a strong discontinuous current and then listens to those given out by wires of the same metals when excited by direct passage of the same current, as revealed by the thread telephone, cannot fail to be struck by the perfect correspondence of the results in both cases. If parity of performance can give any ground for suspecting the same cause, then if the action of the Bell discs be attributed to magnetism, so must also be the action of these sounding wires. This seemed to me at the time convincing enough.

But in addition to De la Rive's magnetic theory, it seemed to me that the sounds originated within the wires, and that they did not need to expend their blows on anything external before sounds were produced. They could be heard when the wires were lying loosely on a table by the aid of a telephone with a wire thread soldered to them. When thrust into the passage of the ear the wires could be heard distinctly without any device for magnifying their loudness. An arc of wire suspended from the thread of a telephone, with each end dipping into adjoining cups of mercury, sounded as loud as if abutting on something solid. Again, it seemed to me unlikely that a wire should receive and divest itself

of its heat as suddenly as it did of its electric charge. A wire takes a sensible time to warm up to the balance of internal gain and external loss of heat and also to lose the heat it has acquired. Now, unless the increments and decrements of length are in the strictest sense momentary, they cannot affect a telephone disc. The clear click that a wire emits when a current begins or ceases in it, both exactly equal in loudness, seemed to me too sharp for the comparatively sluggish course of expansion and contraction by heat and cold. If Professor Chrystal's theory be correct, it can only be the initial increment and decrement of heat that count in this phenomenon, and not subsequent gradual gain or loss. Rapidity of alternation is by no means necessary, for one close or break in a second is as clearly rendered as 500. The extension theory thus appears to me to substitute a thermal for a magnetic onset, and to make it as likely that a sounding clash of molecules accompanies it.

It struck me on considering Professor Chrystal's reasoning on the experiment he exhibited, that if the sound be really in the wire, the skin disc, if placed in the middle instead of at the end of it, would still sound. We should have thus two telephonic receivers, one on each side of the disc. If the sound was due to the push and pull of the wire on getting longer and shorter, the two wires would eliminate the effect the one of the other, but if to internal commotion, little change would be observed whether we had a sounder on one side or on both. So far as I have been able to ascertain the latter is the case. I arranged an experiment in the following way:—*wrefghrw* is a composite thread consisting of very fine platinum wire and fine cotton thread—*ef* and *gh* are the platinum parts. The thread is kept stretched by two equal weights *w*,*w*. The middle of the thread is attached to the skin or paper disc of an ordinary mechanical telephone *dd*. The thread is symmetrically made up on each side of the disc, *ef* and *gh* being exactly equal in every way. At the ends of the platinum wires, wires of copper are soldered which dip into the mercury cups *klm* and *n*, *cz* is a Bunsen battery of six cells. I is a current interruptor or microphone. The one used on this occasion was the mercury break I used for my last communication, vibrating some six times per second. I do not suppose that this deliberate beat, as compared with Professor Chrystal's microphone, which vibrated from 200 to

600 times a second, can affect the result, as what holds for one beat holds for all. I have not beside me a delicate microphone, but so far as I could judge from the rough one in my possession there was no difference in action between the break and it. ABCD is a key the handle of which, by virtue of its elasticity, rests on the stud B, but it can be pressed down on D and thus disconnected from B. The thread is stretched on two rods of glass *rr*, and the telephone may be held in the hand or placed on a supporting board. The current can take two courses according to the position of the handle ABC. As drawn in the diagram it can take the course



*cnhgm*AB*lfek*I*zn*, or if the handle be pressed down, it takes the same as far as A from which it passes to D through K to I and *z*. In the first course both the wires *gh* and *ef* are included, in the latter only *gh*, *ef* being shunted out. To keep the resistance the same in the latter case, a platinum wire *po*, of the same length and thickness as *ef*, is interposed. We have in the first case, as I view it, two opposing receivers on the extension theory or two sounders on the internal click theory. In the second case we have a receiver the same as that of Professor Chrystal. The sound emitted by the disc is much the same in loudness when both wires sound and when only one does; if anything, I fancy it is in favour of the opposing wires. There is also a slight change in sound. When the experiment is so arranged that the wire itself passes through the disc, and we have a continuous wire from *h* to *e*, and when the key is readjusted to take off the current from *m* and lead it back to I, of course through a portion of wire equal to that shunted out

the sound is very much enhanced, arising no doubt from the wire coming in direct contact with the disc. To see if the dipping copper wires had any influence on the sound, I first held them fixed in the hand and found no change. I then took out the mercury and put in a solution of common salt using copper electrodes ; the disc sounded for a beat or two and then ceased, but when the dipping parts were of platinum the beats were quite regular. In this latter case the sound was accompanied by a slight hissing arising from the action of the disengaged gases, but this was quite removed on holding the wires. The sound got when brine or acidulated water is put in the cups is not so loud as with mercury, in consequence of the diminution of current strength.

We do not get the same loudness with this arrangement of the disc in the middle as when only one wire is tightened and the other left loose, for the simple reason that the tightness and consequent elasticity which is favourable to the action of the telephone cannot be so easily got at with a disc balanced between two equal pulls. However, as the common disc is stretched more perfectly the sound rises, so as to leave no doubt, that if the exact tightness were got, the two-pull telephone would be as good as the one with the single pull. Much may be said of the necessary imperfection attending the two-pull telephone. The threads on each side may not be precisely in a straight line, the friction on the rods may not be exactly alike, and that coupled with the comparative fixity of the disc may make a slight difference of pull on opposite sides. But taking all these into account, one would expect that as the conditions approach perfection there would be a corresponding silence, but such is not the case. You may take the telephone into your hand, move it gently about in all directions to secure a position where the loudness is less, but such is not to be found. So far, then, as I can interpret this two-pull telephone, it is something else than the pull of the wire that emits the sound, and the single pull experiment by no means proves the existence of sudden extensions and contractions in the wires. I do not, on the other hand, say that my two-pull experiment disproves them. The conduct of a wire expanding instantaneously in every part may produce an effect like a molecular click, but if so its action must be something very different from the simple pull supposed. A stretched disc with a

stretched thread seems to be capable of rendering all kinds of vibrations as well as those that come normal to its surface.

I also tried if heat or cold on the sounding wires would alter their powers. I endeavoured to apply ice, but unsuccessfully. It was difficult to devise an experiment in which it could be applied and removed so quickly as to produce a contrast. It is also probable that the difference of temperature between that of ordinary air and the freezing point may hardly be great enough to become sensible to the ear. With the Bunsen lamp it was, however, different. It can be easily applied and easily withdrawn. I clamped between two binding screws about 2½ inches of No. 25 platinum wire. I attached a thinner platinum wire to it, which acted as the thread of a parchment telephone pulled transversely. On applying the lamp the sound became sensibly louder and remained so at a white heat. On cooling it again fell off. In the same manner I listened to the effect of a No. 18 soft iron wire. There was a singular increase of loudness just up to the point below which iron became visibly hot, then there was a decided falling off as the wire reached a full red, but it still continued sounding so far as could be judged under the loudness that it had before the lamp was applied. When the lamp was withdrawn the sounds waxed and grew less in reverse order. When the telephone threads were wires of copper and iron the same was observed. When the sound begins to diminish the iron is quite hard, so it is not due to the softening of the iron, which may to some extent account for the falling off at a white heat. In results like these it is possible to discuss the question of molecular impact *versus* expansion, on a new footing. In the case of the platinum the result is much as one would expect, for the rate of increase of its electric resistance and of its expansion are generally allowed to increase at high temperatures. The increase of sound may thus be associated with the one as well as with the other. Iron, however, is exceptional. De la Rive thought that the sounds of wires were in proportion to electric resistance except in the case of iron which stood quite by itself. Here, again, in reference to high temperatures it is quite peculiar. This Society has more than once learned from Professor Tait, and those who have worked with him, of the critical temperature of iron about a dull red heat, in reference to the specific heat of electricity, thermal and electric conductivity,

and anomalous expansion. There is at that point a knot of great complexity and significance. When it is unravelled, there may be something decisive bearing on the present discussion.

Monday, 5th July 1880.

MR ROBERT GRAY in the Chair.

The following Communications were read:—

1. On Peroxides of Zinc, Cadmium, Magnesium, and Aluminium. By J. Gibson, Ph.D., and R. M. Morison, D.Sc.

2. On the Processes in Subepiphysal Bone Growth and some points in Bone Resorption. By De Burgh Birch, M.B., Demonstrator of Physiology in the University of Edinburgh.

(*Abstract.*)

Subepiphysal Bone Growth.—Two processes must be noticed in this connection.

1st, The replacement of the neck of the cartilaginous head or epiphysis by cancellous tissue as an accompaniment to the rise of the epiphysis caused by the growth of the cartilage forming its neck.

The cartilage is channelled by the advancing marrow, the rows of cartilage capsules being opened up.

The opening up of the rows of cartilage capsules results from the presence of a capillary blood-vessel forming the head of the column of marrow which lies in immediate contact with the next unopened capsule (Ranvier). The close proximity into which the pabulum is thus brought with the cartilage corpuscle in the unopened capsule nearest it causes it to grow rapidly and absorb the surrounding cartilage, this occurring, quickest in the direction of least resistance that is, towards the marrow.

The cartilage capsules communicate with each other by means of fine channels, a fact already hinted at by Budge.

The osseous tissue which is deposited upon the cartilage spicules, or septa which result from the channelling of the cartilage, forms the cancellous tissue; this forms a stable base off which the epiphysis

rises by growth of the cartilaginous zone immediately above the primary cancellous spaces.

2nd, The extension of the shaft occurs by opposition to its extremity, thus keeping up with the recession of the epiphysis. The extension of the shaft in length does not lift the head.

The area of proliferation in which these changes occur at the end of the shaft lies in an angular groove at the point where the neck joins the epiphysis, called by Ranvier *encoche d'ossification.*

This author describes fibres in the outer part of the encoche, that is the periosteum, which stretched from the periosteum to the cartilaginous head in which they became lost.

The existence of these is undoubted, and very general.

Origin of the Osteoblasts.—These organisms, which have the function of resorping bone, occur in certain well-marked situations; their origin is from the perivascular connective tissue, *i.e.*, within a short distance of the line of ossification under the epiphysis, and extending over a considerable area, they diminish the number of spicules opening up the cancelli. Under the head of bones, the epiphysis of which have a greater sectional erea than the shaft, and in those positions where the head projects beyond the shaft externally along the interior of the shaft wall.

3. On the Wire Telephone and its Application to the Study of the Properties of strongly Magnetic Metals. By Professor Chrystal.

Four distinct sources of sound were noticed in the course of the experiments.

1. The variation of the longitudinal tension of the wire, owing to variation in the heating, still appears to be the most likely explanation of the action of the wire telephone, when a very fine wire of ordinary metal is used. Experiments were tried with induction coils of various sizes, the violin and microphone being put into the primary circuit and the fine wire telephone into the secondary. It was found that the sound diminished as the spark-giving power of the coil increased. With Professor Tait's large induction coil no sound at all could be obtained, when the secondary was closed through the most sensitive wire I possess.

2. It was found, however, that when the secondary circuit was broken, loud sounds were emitted at the pools of the mercury break. These sounds appear to be due to electrostatic action. They are most probably of the same nature as those obtained in Thomson's singing condenser, Edison's condenser telephone, &c.

3. If the wire of the wire telephone be placed across the lines of force in a strong magnetic field, very loud and pure sounds are obtained when a current interrupted by a tuning-fork is passed through it. These sounds can be obtained with very thick wires of any metal. If a tolerably thin wire be used, although the sound is not much louder, the amplitude of the vibration increases; as much as 2 mm. was observed.

4. Experiments were also made with a view to explain the anomalous behaviour of iron wires established by De la Rive and Dr Ferguson.

Experiments with soft iron wires showed that the sounds did not, in the case of iron, depend in the same way on the length and thickness as they do in the case of ordinary metals, and that their quality is essentially different. The note of the interruptor is often not heard at all, but instead, a variety of other notes are produced, some of them very high accompanied with a fizzing or buzzing noise.

The sound depends on the temperature of the wire, being loudest about a dull red heat, just above the temperature at which the abnormal extension and contraction and the re-glow are usually observed. At higher temperatures the sound falls off very rapidly.

These results suggested that the sound is a consequence of the magnetism of the iron; for, in the case of soft iron, the magnetic susceptibility is at a maximum about the temperature above mentioned, and falls off very rapidly at higher temperatures.

Experiments with steel wires settled the question, for it was found that when the steel was made white hot and then tempered, so as to deprive it of its permanent magnetism and make it hard, it gave no sound at all in the wire telephone. On magnetising it, however, by stroking once or twice with a bar magnet, it sounded quite distinctly, giving a high note and a soft fizzing sound.

The effect of heating a magnetised steel wire is as follows:—At first the sound falls off, first the fizzing disappears, then the high note; then comes an interval of silence; then, as the temperature

increases, the high note comes in again; then the fizzing sound, which quickly rises to a deep buzz accompanied by several notes, among which may be heard the note of the interrupting tuning fork; as the temperature goes on increasing, these sounds die out again in the corresponding order, and when the whole wire is bright red, absolutely nothing can be heard.

All these effects are explained by the magnetism of the steel. The first effect of heat is to destroy the *permanent magnetism*, which about 250° C. is practically insensible; above this temperature the *susceptibility for induced magnetism* increases very fast, reaches a maximum about dull red, and then falls off again.

Advantage was taken of Professor Tait's thermoelectric diagram to verify the close connection between the magnetic, thermoelectric, and other characteristic physical properties of iron and its power of producing sounds, when traversed by a varying current of electricity. The agreement was found to be very striking.

Similar experiments were made with nickel, which is remarkable for the low temperature at which it loses its magnetic susceptibility. The behaviour of a nickel strip in the wire telephone was exactly in accordance with its magnetic properties. The results of thermometric and thermoelectric measurements, rendered the agreement still more remarkable.

Cobalt, when magnetised and heated, gave first a minimum of sound and then an increase; but no maximum was reached at the highest temperature (a bright red), to which I exposed it. This, again, is what is to be expected from its magnetic properties.

Both with cobalt, and with steel which had been softened by heating to a high temperature, the effects due to permanent and to induced magnetism interfere, so that no period of absolute silence appears. Occasionally this interference produces very strong beats. A full account of the experiments above alluded to will be published in "Nature" (vol. xxii. No. 561, p. 303—July 29, 1880).

In the thermo-electric measurement above referred to I had the able assistance of Dr Knott, whose experience in such work is well known to the Society. The curves from which the above references were drawn were constructed by him, and will be given in the detailed account of the experiment to be published in "Nature."

4. Notice of the Completion of the new Rock Thermometers at the Royal Observatory, Edinburgh, and what they are for. By Professor Piazzi Smyth, F.R.S.E.

The nature of this paper may be understood from the following headings :—

(1.) The making and placing of the new thermometers.

(2.) Practically described by Mr Thomas Wedderburn.

(3.) The problem with the old thermometers.

(4.) Their next use in level fluctuations.

(5.) Their employment by Sir Wm. Thomson.

(6.) Their subsequent demonstration of the cycle of supra-annual waves of heat and cold.

(7.) The published predictions in 1872 for 1878–80.

(8.) The spoiled predictions in 1877, under the influence of erroneous sun-spot dates.

(9.) The rectified predictions in 1879, when the true date of sun-spot minimum was ascertained by direct observation.

(10.) How to obtain correct dates for future sun-spot minima.

Appendix I.—The contract for the new thermometers.

Appendix II.—Account of works by Mr Richard Adie.

Appendix III.—Further account by Mr T. Wedderburn, of Adie & Son.

Appendix IV.—The cyclical seasons of 1878–80, as predicted in 1872.

Appendix V.—Scottish meteorological data from 1821–1880, arranged in quadruple annual means for cyclical inquiries.

Plate representing the above numerical tables, graphically.

Monday, 19*th July* 1880.

Professor Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read :—

1. Report on Fossil Fishes collected by the Geological Survey of Scotland in Roxburghshire and Dumfriesshire. Part I. —Ganoidei. By Dr R. H. Traquair.

2. On Some New Crustacea from the Cementstone Group of the Calciferous Sandstone Series of Eskdale and Liddesdale. By B. N. Peach, F.G.S., of the Geological Survey of Scotland. Communicated by Professor Geikie.

(*Abstract.*)

The species enumerated in this paper belong to the two orders Phillopoda and Decapoda.

Of the Phyllopods, the author describes two species of *Ceratiocaris* (Salter), which differ from their Upper Silurian allies in the enormously-developed abdomen and in the small size of the carapace, also in the comparative insignificance of the side spines of the tail compared with the telson. As far as the author is aware, these are the first obtained from the Calciferous Sandstone series, although carapaces of *Ceratiocaris* have been got from the Mountain Limestone of England. (*C. Scorpioides*, $1\frac{1}{2}$ to 2 inches long; *C. elongatus*, 5 to 8 inches long.)

Of the Decapods, seven new species are described, viz., five belonging to the genus *Anthrapalæmon* (Salter), one belonging to the genus *Palæocrangon* (Salter). These do not differ in any essential respect from the recent Macrurous Decapods, and one belonging to *Palæocaris* (Meek and Worthen)—a genus which, as far as the author is aware, has hitherto only been got from the Illinois Coalfield, in the United States, and is represented by only one species, the *Palæocaris typus* (Meek and Worthen). He proposes to call the present one *P. Scoticus.* This is a most interesting creature, for the carapace extends only over the cephalic region, while the thoracic segments are all free and movable, yet its cephalic appendages and the character of the tail show it to be most nearly allied to the Macrurous Decapods.

3. Gaseous Spectra in Vacuum Tubes. By Piazzi Smyth, F.R.S.E., &c.

(*Abstract.*)

The work described in this paper consists of rather careful measurements, but under low dispersion only, of the spectra of twenty gas-vacuum tubes, generally of different gases and illuminated

by small induction sparks, but seen very brightly by the end-on method of viewing.

A comparison of the different spectra thus obtained follows, and some curious results are elicited as to the prevalence of certain impurities among gases, as well as alterations and even transformations of some of them with time and use.

These facts are contained chiefly in Appendix 1 and Appendix 2; while a third appendix, kindly contributed by Professor Alexander Herschel, contains some further observations of his with the same apparatus but higher dispersion introduced. See his account of the same.

Two plates of spectra accompany the paper.

4. On the Diffusion of an Impalpable Powder into a Solid Body. By R. Sydney Marsden, D.Sc., F.R.S.E., &c.

In a note on "The Effect of Heat on an Infusible, Impalpable Powder," by Professor P. G. Tait, in the Proceedings of the Royal Society of Edinburgh, vol. ix. p. 298, for the year 1876–77, Professor Tait points out that such a powder becomes very fluid under the action of heat, and behaves in many respects in the same way as a liquid would do—viz., convection currents are distinctly to be observed, and small particles of the powder are thrown up from the surface, in the same manner as we perceive little drops of water thrown up from the surface of a glass of soda-water. And Professor Tait then asks the question—If, supposing we had two such infusible, impalpable powders, would they diffuse into one another as do gases and liquids? This is a question which as yet has not been answered. Professor Tait and I have been engaged in some experiments on the subject for some time, but the difficulties (chemical and physical) to be overcome are much greater than at first sight appear, and at present we are unable to say definitely whether they do so or not. But I think an answer may be obtained from another source. In some recent experiments I had occasion to have a number of Berlin porcelain crucibles and amorphous carbon in an impalpable powder kept in contact with each other at very high temperatures for from ten to twelve hours, with the following effect, that, although the crucibles did not become fused, but

retained their exact original form, yet the carbon found its way to a considerable distance into the crucible, and some of the particles penetrated the crucible throughout. This was not a case, therefore, of fusion and mechanical mixture.

On examining a section of the crucible under the microscope, the particles of carbon can be distinctly seen disseminated through the silica and alumina of the crucible, and thickest in the glaze and outer parts which are nearest to the carbon. We also notice a number of "tricites" all along the juncture of the alumina with the glaze of the crucible, arising from the devitrification of the glaze, and a number of particles of larger size which are contained in the original crucible. On examining a section of the crucible before being used we see nothing in the form of little black particles disseminated throughout, and are thus able to recognise more completely these black specks as carbon, the result of diffusion.

Now carbon, so far as we know, has no chemical action on silica or alumina, and consequently it cannot have been taken into the crucible by chemical action. This, then, is a distinct case of the diffusion of an impalpable powder into a solid body in a softened state. And it has the advantage that, the solid body being transparent, we can, by examining it with the microscope, see what has actually taken place. And here it gives us an insight into another matter. It is evident that this is precisely what takes place in the conversion of bar iron into steel by the cementation process. The carbon in the state of an impalpable powder diffuses into the bars of iron whilst they are in the softened state, the operation taking a number of days before it is completed. Thus it seems to me to explain the up to the present time unsettled question of the conversion of iron into steel by the cementation process, and to render unnecessary the "Occlsuion of Gases" theory.

In order to make absolutely certain of the fact that carbon had really penetrated into the crucible, I took a portion of the crucible, pounded it down, and treated it with hydrofluoric acid for some days. I then filtered off the insoluble residue which was left, and after treating it successively with hydrochloric acid and soda, ultimately, on largely diluting, got the carbon (in an exceedingly fine state) suspended in the water, and by decantation, and filtering the decanted fluid, got it on to a filter. Its quantity, however, in this

extreme state of division was not large enough for me to get it off the filter and examine it further; but after the treatment which it had received—it still remaining a brownish-black powder—there can be no doubt of its being carbon.

A similar case of diffusion takes place on a small scale when we hold a cold porcelain lid over a bunsen flame, when, as is well-known, we obtain a black deposit under the glaze of the porcelain without the latter being fused. Here the carbon in the impalpable condition diffuses itself into the porcelain, but aided by the convection currents of the gases of the lamp.

5. On the Variation with Temperature of the Electric Resistance of certain Alloys. By Professor J. G. MacGregor and C. G. Knott, D.Sc.

6. Preliminary Report on the Tunicata of the "Challenger" Expedition. Part II. By W. A. Herdman, D.Sc.

(*By permission of the Lords Commissioners of the Treasury.*)

Since the publication of the first part of this preliminary report (Proc. Roy. Soc. Edin., 1879–80, p. 458), I have received some additional specimens of Ascidians belonging to the "Challenger" collection, and including the following Ascidiadæ.

Ascidia cylindracea, n. sp.

External appearance.—Shape nearly cylindrical; posterior end rounded and wider than truncated anterior end; ventral edge nearly straight, dorsal slightly concave. Attached by base and lower half of left side. Both apertures at anterior end; branchial towards ventral side, sessile; atrial on dorsal edge, forming a rounded projection; both distinctly lobed. Surface smooth. Colour yellowish-grey. Length, 2 cm.; breadth, 1·2 cm.

Test of moderate thickness, transparent, showing vascular ramifications.

Mantle having well-marked muscular bands.

Branchial sac extremely delicate; vessels very slender. Stigmata long and narrow, some being twice as long as others in consequence

of the alternate transverse vessels being interrupted here and there, and sometimes altogether wanting. Internal longitudinal bars narrow but well-marked, having papillæ at the angles of the meshes; generally three stigmata in a mesh.

Tentacles very long and numerous, their bases almost touching.

Dorsal lamina plain; no ribs or teeth.

One specimen from Station 163 (Twofold Bay, Australia); 120 fathoms.

Ascidia despecta, n. sp.

External appearance.—Shape oval; the anterior end being narrow while the posterior is wider and rounded. Dorsal edge rather more convex than ventral. Attached by posterior half of left side. Branchial aperture near anterior end; atrial not distant, on dorsal edge about one-fourth of the way down. Surface covered with small soft projections giving a rough appearance. Colour grey. Length, 1·7 cm.; breadth, 1 cm.

Test thin, nearly transparent, showing fine vascular ramifications. Trunks enter near centre of area of attachment. Test prolonged into a few short tufts near base of left side.

Mantle normal.

Branchial sac not plicated, rather stout. Internal longitudinal bars strong, bearing large papillæ at the corners of the meshes, no smaller intermediate ones; three or four stigmata in a mesh.

Tentacles large and numerous, all one length.

Dorsal lamina wide, transversely ribbed; margin plain.

One specimen from Kerguelen Island; 10 to 100 fathoms.

Ascidia nigra, Savigny.

Three specimens from Station 142? (south of Cape of Good Hope); 150 fathoms.

Ascidia pyriformis, Herdman.

A large specimen from Port Jackson; 6 fathoms.

Ascidia placenta, n. sp.

External appearance.—Shape elongate, elliptical or oval; flattened laterally; the anterior end slightly the narrower, posterior

end rounded. Attached by a small area a little posterior to the middle of the left side. Apertures both on right side, inconspicuous, sessile: branchial median and nearly terminal; atrial a short distance from the dorsal edge, more than one-third of the way down, lobes indistinct. Surface slightly wrinkled and approaching to velvety. Colour yellowish-grey or horn-colour. Length, 6·5 cm.; breadth, 4 cm.

Test rather thin, soft, easily torn, roughish about base of attachment. Inner surface smooth and glistening; vessels feebly developed.

Mantle moderately muscular.

Branchial sac very delicate, minutely plicated; stigmata long and thin, eight to twelve in a mesh. Papillæ long and curled; smaller intermediate ones also present, and in some places connected by fine transverse vessels.

Dorsal lamina slightly ribbed transversely, with a large tooth at the end of each rib and three or four smaller intermediate ones.

Tentacles filiform, about 24 in number, all the same length.

Olfactory tubercle longish elliptical, with the opening at the anterior end.

Two specimens from Station 150 (south of Kerguelen Island); 150 fathoms.

This species resembles *Ascidia tenera* considerably in external appearance, but is quite distinct.

Corella japonica, Herdman.

Three specimens from Kobé, Japan; 8 to 50 fathoms.

II. Clavelinidæ.

The little group of Social Ascidians is here placed next to the Ascidiadæ as a fourth family of *Ascidiæ simplices.* The old name Clavelinidæ is retained, the only change being that, instead of occupying a position intermediate between the Simple and Compound Ascidians, they will now be included in the former group. As the explanation of my reasons for making this change in classification necessitates frequent reference to former observations and theories, it is simpler, and seems more advantageous, to give the argument in the form of a brief outline of the history of the group.

The first Social Ascidians known to science were two species of *Clavelina*, viz., *C. borealis* and *C. lepadiformis.*

The first of these, *Clavelina borealis,* was described under the name *Ascidia clavata* by Pallas* in 1774, and was referred to by Bruguiere† in 1789. It was afterwards, in 1815, described at greater length under the same name by Cuvier,‡ who united it with *Ascidia* (now *Ciona*) *intestinalis* to form his fourth tribe of the genus *Ascidia.*

The second species, *Clavelina lepadiformis,* was observed by Müller§ and described by him in 1780 under the name of *Ascidia lepadiformis*; Bruguiere|| (1789) mentions this species also.

In 1816 Savigny¶ founded the genus *Clavelina* for the reception of these two species, which he separated from *Phallusia* (*Ascidia*) on account of their being pedunculated. He still retained them, however, in the Simple Ascidians. In his third memoir (p. 109) he gives an account of *Clavelina borealis,* and states (p. 110) that "Les véritables rapports des Clavelines sont avec les Phallusies." In his systematic table (p. 171) he places *Clavelina* as the last genus of the Simple Ascidians next to *Phallusia,* and immediately following the *Phallusiæ Cionæ* (*C. intestinalis*).

In the same year (1816) Lamarck** places these two species, *C. clavata* and *C. lepadiformis,* in the genus *Ascidia.*

It is evident, then, that those of the older naturalists to whom any of the Clavelinidæ were known included them unhesitatingly in the Simple Ascidians. It must be remembered, however, that although Gaertner was acquainted with *Botryllus* and *Distomus* in 1774, and Renieri (1793) was to a certain extent aware of the true nature of some of these forms, yet the Compound Ascidians were hardly recognised as such till after the appearance of Savigny's well-known memoirs. By Cuvier, Savigny, and Lamarck, however, to all of whom the Compound Ascidians were well known, *Clavelina* was considered a Simple Ascidian closely allied to *Ciona intestinalis.*

* Spicilegia Zoologia, fasc. 10, pl. i. fig 16.
† Encyclopédie méthodique, pl. lxiii. fig. 11.
‡ Mém. du mus. d'hist. nat., t. ii. pl. ii. figs. 9, 10.
§ Zoologia Danica, Pt. ii. p. 119., tab. lxxix. fig. 5.
|| *Loc. cit.*, pl. lxiii. fig. 10.
¶ Mémoires sur les animaux sans vertèbres, Pt. ii., fasc. 1, p. 87.
** Histoire naturelle des animaux sans vertèbres, t. iii. p. 126.

In 1834, J. J. Lister, F.R.S.,* published a paper entitled "Some Observations on the Structure and Functions of Tubular and Cellular Polypi and of Ascidiæ," in which he gave an account of a small species of Ascidian, afterwards described by Wiegmann† as *Perophora listeri.*

Lister pointed out the condition in which the individuals lived, the fact that each possessed a complete set of organs of its own, but that all were connected by a common circulatory system; and stated that "it increases by sprouts: the two streams of the stem run through the bud before its organs are developed."‡

Clavelina remained in the *Ascidiæ simplices* till 1842, when Milne-Edwards published his celebrated "Observations sur les Ascidies composées des côtes de la Manche."§

In this elaborate work he gives an account of several species of *Clavelina*, and proposes that that genus, along with *Perophora*, should be separated from both Simple and Compound Ascidians, and form an independent intermediate group, to which he gives the name of *Ascidiæ sociales.* This group he defines as comprising ascidians which reproduce by buds as well as by eggs, and which live united by common radiciform prolongations, but which otherwise are free of all adhesion to one another. "On réserverait alors le nom d'*Ascidies simples* pour les Ascidies qui ne se reproduisent point par bourgeons, et qui ne vivent pas réunies en groupes, par l'intermédiaire d'une portion commune du tissu tégumentaire.

Enfin, les *Ascidies composées* se rapprocheraient de cette division nouvelle par leur mode de multiplication, mais s'en distingueraient par l'existence d'un seul corps tégumentaire commun à tous les individus dont se compose chaque colonie; tandis que chez les premiers, chaque individu possède une tunique tégumentaire qui lui est propre."||

Milne-Edwards' ground for separating the Social from the Simple Ascidians was twofold; first the union of the individuals by stolons, and secondly the power they possess of reproducing by gemmation. Of course these two points are really only one, as the union is

* Phil. Trans., 1834, Pt. ii. p. 365.
† Wiegmann's Archiv, 2 Bd., 1835, p. 309.
‡ *Loc. cit.*, p. 382.
§ Mém. Inst. France, vol. xviii. p. 217.
|| *Loc. cit.*, p. 266.

simply the result of the gemmation, and taken alone is not a characteristic of any importance.*

The power of reproducing by gemmation is of more value, and seems at first sight to form a distinction between the Clavelinidæ and the other Simple Ascidians; this, however, is more apparent than real. The buds on the stolons of the Clavelinidæ are developed from the ends of the blood-vessels, and are at first merely slight enlargements similar to and comparable with the knobs on the end twigs of the vessels in the test of an *Ascidia;* these last vessels being homologous with those in the stolons of the *Clavelina.* In *Ascidia* the vessels do not project beyond the test, but in *Molgula* they are prolonged considerably as hair-like simple or branched processes,† and in *Ciona,* at the base of the test, projections exactly like the stolons of *Clavelina,* having the same structure and containing similar blood-vessels, frequently grow out over the object to which the individual is attached.

It thus appears that all the apparatus necessary for budding is present in the Simple Ascidians as well as in the so-called Social, and that in the former it may even go the length of forming stolons, but these have never been seen to develop into new individuals.

Philippi ‡ in 1843 gave a short account of an Ascidian he had found at Naples, and which he called *Rhopalœa neapolitana.* This form is elongated, somewhat like a *Clavelina* in shape; the branchial aperture, however, is eight-lobed, and the atrial six-lobed as in *Ascidia.* "Im obern Drittheil etwa, wo die Verdickung anfängt merk licher zu werden, sassen in einem unregelmässigen Kranz zweispaltige und dreispaltige Auswüchse, jungen Ascidien nicht unähnlich." The body is divided into thorax and abdomen joined by a narrow neck; the heart is placed on the right side of the intestinal loop, and the

* If the mere fact of the union of individuals, irrespective of the cause of that union, is to be considered an important point, then aggregations of true and undoubted Simple Ascidians of the genera *Ascidia* and *Cynthia* must also be considered colonies of Social Ascidians, as they were in the case of *Styela grossularia* by Van Beneden in 1847 (Mém. de l'Acad. roy. de Belgique, t. xx.). It is now well known that these aggregations are merely caused by the proximity and the coalescence of the tests, and indicate no relationship whatever between the different individuals.

† For an explanation of the true nature of these hair-like processes in the Molgulidæ, see Lacaze-Duthiers, Arch. de Zool. expér. et gén. vol. iii. p. 314 (1874).

‡ Müller's Archiv für Anatomie, 1843, p. 45.

ovary on the left just as in *Clavelina*. The branchial sac, however, is provided with strong papillæ. The dorsal lamina finally is formed of languets.

This is a very interesting form, being clearly intermediate in its characters between *Clavelina* and *Ciona*. The external shape and the condition of the dorsal lamina would allow of its being placed in either genus. The presence of thorax and abdomen, and the position of the heart and ovary, ally it to *Clavelina*, while the lobes round the branchial and atrial apertures, and the papillæ on the branchial sac, show its relationship to the ASCIDIADÆ. Finally, the bud-like projections from the test, a careful investigation of which would have been valuable, seem in the figure very like young individuals, and, if they are so, indicate gemmation probably from the blood-vessels of the test.

Adams * in 1858 placed Philippi's *Rhopalæa* in the old genus *Clavelina*. This, I think, is quite wrong. *Clavelina* has no lobes round its apertures, and no papillæ on its branchial sac, while *Rhopalæa* has both; still the fact showed that the resemblance of this form to the CLAVELINIDÆ was detected, although it had been described by Philippi as a Simple Ascidian.

Bronn † (1862) follows Milne-Edwards, and divides the *Ascidiæ* into *simplices*, *sociales*, and *compositæ*.

Claus ‡ (1876) unites the ASCIDIADÆ and the CLAVELINIDÆ as one group, "*Einfache und aggregirte Ascidien*," distinct from the compound Ascidians (*Zusammengesetzte Ascidien*).

Professor Giard in his "Recherches sur les Ascidies Composées ou Synascidies" § (1872) unites the Social Ascidians with the Compound, including *Clavelina* and *Perophora* in the *Synascidiæ*. This he does chiefly on account of their property of budding, although he admits that budding alone is not sufficient to separate the Social from the Simple Ascidians. He gives as the characteristics of his *Synascidiæ* ||:—Reproduction by gemmation, stigmata oval, the embryo being developed rapidly, and being almost complete before being hatched.

* Genera of Recent Mollusca, vol. ii. p. 595.

† Klassen und Ordnungen des Thier-Reichs, B. III. p. 216.

‡ Grundzüge der Zoologie, p. 840.

§ Arch. de Zool. expér. et gén., vol. i. p. 501.

|| *Loc. cit.*, p. 603.

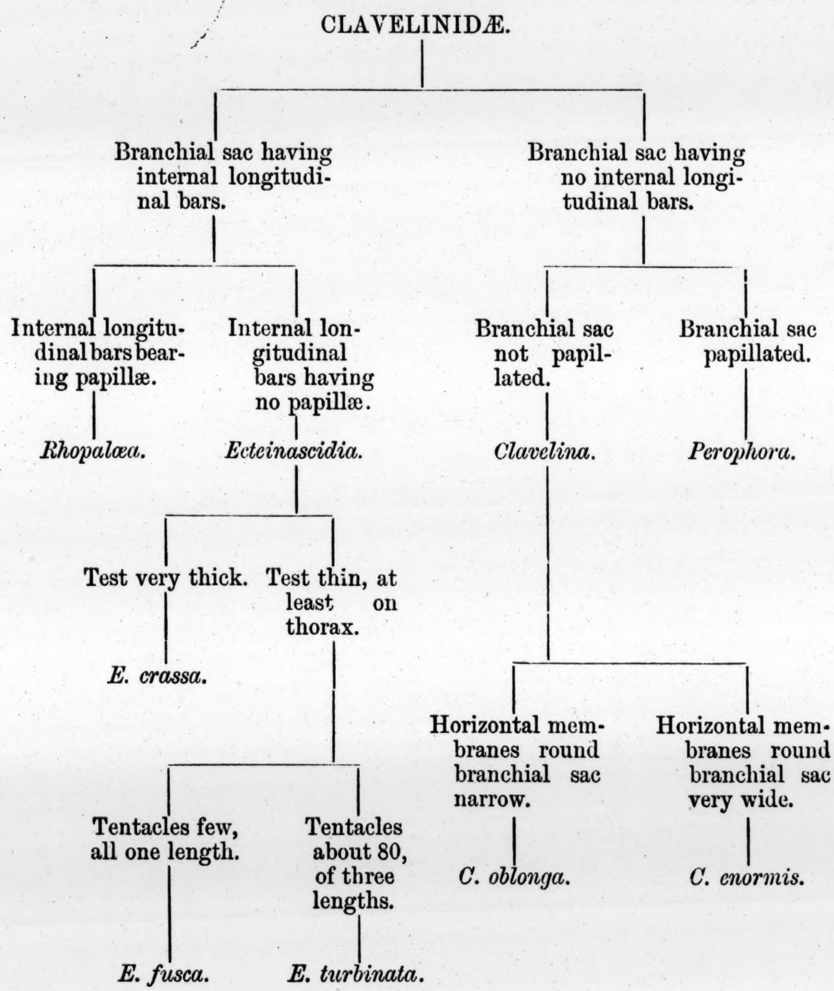
The first of these characters he had admitted to be insufficient alone, and I am unable to recognise the value of the other two. It is difficult to understand how the shape of the stigmata can be a characteristic of much value, and the statement that oval stigmata are characteristic of the *Synascidiæ* may be easily refuted, as many Simple Ascidians have oval stigmata, while a species of *Colella* (*Aplidium pedunculatum*, Q. and G.), an undoubted Synascidian, has very long slit-like stigmata with parallel sides. The stage of development in which the embryo is hatched cannot be considered of much importance, as it seems to vary in closely-allied forms; and Giard's generalisation that only in Compound Ascidians does the embryo remain in the egg-membrane till far advanced in development, will certainly not hold, as Kupffer * describes and figures the embryo of *Molgula macrosiphonica* as being still, when almost completely developed, covered by the "eihaut." This is also the case in several others of the few Simple Ascidians, the development of which has been observed.

In conclusion, it appears that the power of reproducing by gemmation is the only difference of more than generic rank between *Clavelina* and *Ciona*, while *Ecteinascidia*, a new genus of the CLAVELINIDÆ might, were it not for the fact that it reproduces by budding, and that the individuals are united into colonies, be included in *Ciona*.

In *Clavelina* in an adult colony, the stolons connecting the bases of the individuals often atrophy and in places entirely disappear, leaving the individuals without any connection. They are now practically Simple Ascidians. In *Ecteinascidia*, the same seems to happen; and I believe that if a *Ciona intestinalis*, a solitary *Clavelina lepadiformis*, and a solitary *Ecteinascidia turbinata* were submitted to a naturalist who did not know the several species, he would declare that they were all Simple Ascidians, that the *Ecteinascidia* and the *Ciona* were species of the same genus, and that the *Clavelina* was a nearly allied one. This would be the natural arrangement were it not for the budding, which, however, should be considered of sufficient importance to characterise a family, and, therefore, I unite those Simple Ascidians which reproduce by gemmation and form colonies, including the genera *Clavelina*, *Ecteinascidia*, *Perophora*

* "Entwickelung der einfachen Ascidien," Archiv für Microscopische Anatomie, 1872.

and possibly *Rhopalœa*, as the family Clavelinidæ, and place them next to the Ascidiadæ in the *Ascidiæ simplices.*



Ecteinascidia, n. gen.

External appearance.—Shape oblong, tapering posteriorly.

Branchial sac having internal longitudinal bars, but no papillæ.

Dorsal lamina reduced to languets.

Tentacles simple.

Viscera extending beyond branchial sac posteriorly.

This genus is formed for the reception of three species which seem to be intermediate in their characters between *Ciona* and *Clavelina*, and, except in one point, resemble Philippi's *Rhopalœa* more than any hitherto described form.

Ecteinascidia must, on account of its property of forming colonies by gemmation, and having no papillæ on its branchial sac, be included in the Clavelinidæ, but it differs from *Clavelina* in possessing well-marked internal longitudinal bars. In this last character it approaches *Ciona* and *Rhopalœa*, from both of which it differs in the absence of papillæ.

Ecteinascidia, crassa, n. sp.

External appearance.—Shape irregular, rudely triangular; attached by extended base to clump of sponge spicules. Anterior end more or less rounded; sides irregular. Both apertures sessile, near or at anterior end. Surface rather irregular. Colour yellowish-grey. Length, 2 cm.; breadth along base, 3·5 cm.

Test enormously thickened.

Mantle strongly developed. Muscle bands thick.

Branchial sac crumpled. Internal longitudinal bars fine, undulating, borne on large pyramidal ducts. No papillæ. Stigmata elongated.

Dorsal lamina languets.

Viscera extending considerably beyond branchial sac, and forming a distinct abdomen.

Two specimens attached to the spicules of a large sponge (*Laburia hemisphærica*) from Station 192 (Ki Island); 129 fathoms.

Ecteinascidia fusca, n. sp.

External appearance.—Individuals united by a short, thick, irregular stolon, which looks merely like a continuation of their posterior extremities. Shape very elongated, some specimens rudely club-shaped; anterior end wide, truncated; posterior half narrower, contorted, passing down into the stolon. Apertures nearly terminal, both placed on the right side of the extremity; branchial near the middle; atrial near the dorsal edge. Surface smooth but uneven, especially at the posterior end, where knobs and processes are formed. Colour dark brown. Length, 5 cm.; breadth, 1·5 cm.

Test thickish, especially on the posterior part; vessels present.

Mantle thin; muscular fibres distant, but well marked, and of a reddish-brown colour.

Branchial sac delicate. Internal longitudinal bars narrow but distinct, undulating, supported by broad horizontal membranes, provided with triangular flaps, to which they are attached. No papillæ. Stigmata longish, elliptical; about three in a mesh.

Dorsal lamina reduced to languets.

Tentacles filiform, few and distant.

Olfactory tubercle irregularly oval-shaped.

Viscera prolonged posteriorly to the branchial sac, and extending into the posterior narrow part of the body.

One colony (several individuals) from Banda; 17 fathoms.

Ecteinascidia turbinata, n. sp.

External appearance.—Many individuals united by a delicate much-branched stolon. Shape elongated, sometimes almost pyriform; anterior three-fourths of much the same width, posterior end narrowing rapidly to a short slender stalk continuous with the stolon. Both apertures on the right side of the anterior end, sessile. Surface smooth. Colour light yellowish-brown. Length, 3 cm.; breadth, 1 cm.

Test thin and membranous, transparent.

Mantle thin.

Branchial sac simple. Internal longitudinal bars narrow, but well marked. No papillæ. Stigmata elliptical, rather long.

Dorsal lamina reduced to languets, small and rather distant.

Tentacles filiform; of three lengths, placed alternately, about twenty of the long and medium sizes and forty of the short. They are placed thus:—long, short, medium, short, long, &c.

Olfactory tubercle elongated posteriorly, so as to be carrot-shaped.

Viscera extending slightly beyond the branchial sac posteriorly.

One large colony from Bermuda. Shallow water.

Clavelina oblonga, n. sp.

External appearance.—Individuals closely united by their posterior extremities, which form thick irregular stolons. Shape irregularly oblong, or sometimes club-shaped; anterior end wide and rounded;

posterior generally very narrow. Both apertures at anterior end, sessile, not lobed. Surface smooth, with occasional transverse wrinkles, especially on posterior end. Colour light yellowish-grey, nearly white. Length, 2 cm, ; breadth, ·7 cm.

Test thin, especially at anterior end ; transparent.

Mantle moderately strong.

Branchial sac simple, transverse vessels of one width, bearing horizontal membranes ; no internal longitudinal bars ; stigmata short, elongate-elliptical.

Dorsal lamina.—Languets of moderate size.

Tentacles short and stout, about twenty in number, alternately long and short.

Olfactory tubercle small, irregularly oval in outline.

One colony from Bermuda. Shallow water.

Clavelina enormis, n. sp.

External appearance.—Individuals united by their bases to form an irregular mass. Shape rudely oblong, with both apertures at anterior end. Surface smooth but irregular, especially on the posterior part. Colour greyish, with a slight brown tinge. Length, 3 cm. ; breadth, ·7 cm.

Test moderately thin on the anterior half ; posteriorly thicker, wrinkled, and encrusted with sand.

Mantle well developed.

Branchial sac.—Transverse vessels all one size, with wide horizontal membranes hanging from them. Stigmata regular, short, and narrow, with rounded ends. Fine longitudinal vessels (interstigmatic) strong.

Dorsal lamina.—Languets large, close, and numerous.

Tentacles stout, long and short alternately ; about twelve of each.

A single colony of four adult individuals and several buds. The united posterior ends form an irregularly-shaped base, which adhered to the surface of a mass of *Balani*, *Synascidiæ*, &c. Two of the adult individuals are united together along one side, so that their tests form a common investing mass.

I am convinced that this is a pathological specimen, that the adhesion of the two individuals is abnormal, that the irregular

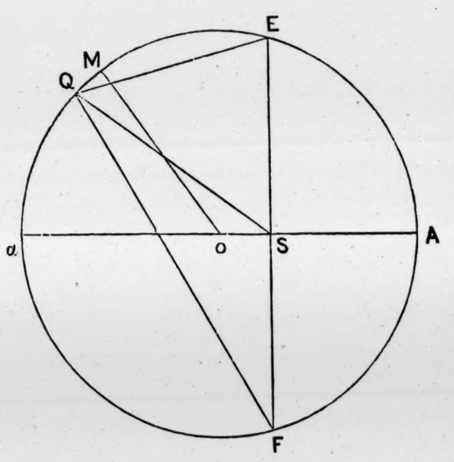
stem-like base is a hypertrophy due to the irregular surface the colony was attached to, and that, therefore, this species cannot with certainty be separated from *Clavelina*.

Simon's Bay; 10 to 20 fathoms.

7. Description of New Astronomical Tables for the Computation of Anomalies. By Mr Edward Sang.

(*Abstract.*)

The planets move round the sun in ellipses, in such a manner that the radii vectores describe areas proportional to the times. Now, by means of parallel lines, we can always project an ellipse upon a plane surface so as to make the projection circular, and thus we have to consider the motion of a point in the circumference of a circle, describing round an excentric point areas proportional to the times. If we take S for the excentric point, that is for the projection of the sun, and suppose Q to be the projection of the planet's place, the area ASQ is proportional to the time elapsed since the perihelion passage. The angle AOQ is called, very inappropriately, the excentric anomaly; I prefer to call it the angle of position. If we suppose a point M to move uniformly along the circumference, with the periodic time of the planet, and to have reached M when the actual projection of the planet is at Q, it is clear that the sector AOM must be equivalent to the area ASQ. The angle AOM is the mean anomaly.



Having drawn ESF perpendicular to the diameter ASOa, join QE and QF; then it is evident that the surface EQFA is halved by the compound line ASQ; wherefore the area ASQ passed over by the radius vector is half the sum or half the difference of the circular segments QAF and QE, according as Q lies beyond AE or within it.

Denoting the arc AE by e, and the arc of position AQ by p, and

observing that the area AOM is equivalent to ASQ, we have, denoting the segment AOM by m,—

$$2m = \text{segm.}\ (p+e) + \text{segm.}\ (p-e),$$

and thus the determination of m from p, or of p from m, is to be accomplished by help of a table of circular segments, which must be measured, not in parts of the square of the radius, but in degrees of the surface of the circle.

For the purpose of rendering this exceedingly simple formula available for actual calculation, a table was constructed of the sines for each minute of the quadrant, measured in degrees of arc; by its help the values of the circular segments for each minute of the whole circumference were written out, true to within one ten-thousandth part of a second of the modern division.

When we have got a tolerable first approximation, this table enables us to compute the position corresponding to a given mean anomaly by a simple proportion.

In order to guide us to a first assumption, tables were constructed of the mean anomalies corresponding to each degree of position from 0° C. to 200° C., and for every value of e from 0° C. to 100° C., with their differences and variations, true to the nearest second; and thus, in every possible case, the solution of Kepler's problem is obtained in a few minutes, true to far within the hundredth part of a second of the new division.

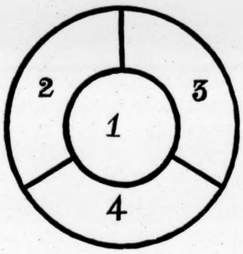
For the construction of these tables, one million six hundred thousand figures were written, and of these the three volumes placed on the table contain about twelve hundred thousand. If the ancient division of the quadrant had been used, the labour would have been more than doubled.

8. The Discharge of Electricity through Olive Oil. By A. Macfarlane, D.Sc., and P. M. Playfair, M.A.

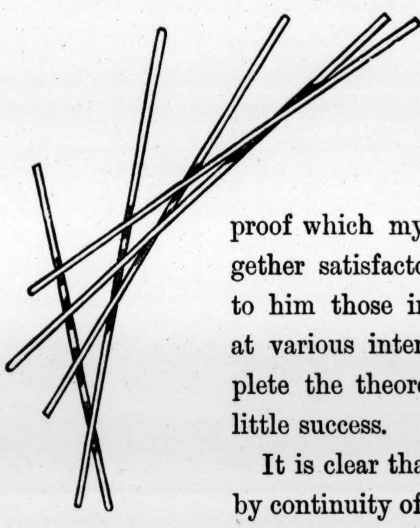
9. Note on the Colouring of Maps. By Frederick Guthrie.

From the Proceedings of the Royal Society of Edinburgh, No. 106, p. 501, it appears the colouring of maps is receiving attention. This note bears chiefly upon the history of the matter.

Some thirty years ago, when I was attending Professor De Morgan's class, my brother, Francis Guthrie, who had recently ceased to attend them (and who is now professor of mathematics at the South African University, Cape Town), showed me the fact that the greatest necessary number of colours to be used in colouring a map so as to avoid identity of colour in lineally contiguous districts is four. I should not be justified, after this lapse of time, in trying to give his proof, but the critical diagram was as in the margin.



With my brother's permission I submitted the theorem to Professor De Morgan, who expressed himself very pleased with it; accepted it as new; and, as I am informed by those who subsequently attended his classes, was in the habit of acknowledging whence he had got his information.



If I remember rightly, the proof which my brother gave did not seem altogether satisfactory to himself; but I must refer to him those interested in the subject. I have at various intervals urged my brother to complete the theorem in three dimensions, but with little success.

It is clear that, at all events when unrestricted by continuity of curvature, the maximum number of solids having superficial contact each with all is infinite. Thus, to take only one case, n straight rods, one edge of whose projections forms the tangent to successive points of a curve of one curvature, may so overlap one another that, when pressed and flattened at their points of contact, they give $n-1$ surfaces of contact.

How far the number is restricted when only one kind of superficial curvature is permitted must be left to be considered by those more apt than myself to think in three dimensions and knots.

10. Remarks on the previous Communication. By Prof. Tait.

(*Abstract.*)

In a paper read to the Society on 15th March last (*ante*, p. 501), I gave a series of proofs of the theorem that four colours suffice for a map. All of these were long, and I felt that, while more than sufficient to prove the truth of the theorem, they gave little insight into its real nature and bearings. A somewhat similar remark may, I think, be made about Mr Kempe's proof.

But a remark incidentally made in the abstract of my former paper has led me to a totally different mode of attacking the question, which puts its nature in a clearer light. I have therefore withdrawn my former paper, as in great part superseded by the present one.

The remark referred to is to the effect that, if an even number of points be joined, so that three (and only three) lines meet in each, these lines may be coloured with *three* colours only, so that no two conterminous lines shall have the same colour. (When an odd number of the points forms a group, connected by *one* line only with the rest, the theorem is not true.)

This follows immediately from the main theorem when it is applied to a map in which the boundaries meet in threes (and the excepted case cannot then present itself). For we have only to colour such a map with the colours A, B, C, D. Then if the common boundaries of A and B, as also of C and D, be coloured α; those of A and C, and of B and D, β; and those of A and D, and of B and C, γ, it is clear that the three boundaries which meet in any one point will have the three colours α, β, γ.

The proof of the elementary theorem is given easily by induction; and then the proof that four colours suffice for a map follows almost immediately from the theorem, by an inversion of the demonstration just given.

We escape the excepted case by taking the points as the summits of a polyhedron, all of which are trihedral; and when the figure is a pentagonal dodecahedron the theorem leads to Hamilton's *Icosian Game.*

11. Note on the Wire Telephone as a Transmitter. By James Blyth, M.A.

It was shown some time ago by Dr Ferguson, and more recently by Professor Chrystal and Mr Preece, that a fine wire attached to a mechanical telephone can act very well as a receiver in a telephonic circuit, provided a make and break, or some form of microphone transmitter, be employed. None of these experimenters, however, have said anything about the action of such a wire as a transmitter. Being struck by the convertibility, in general, of all forms of telephone receivers into transmitters, and *vice versa*, it occurred to me to try how far this wire telephone, as it has been called, could be made to act as a transmitter to an ordinary Bell telephone as receiver. I was much interested to find that it could act in that capacity wonderfully well, as thereby a new element of some importance is introduced into the discussion of the real cause or causes of the action of the wire telephone whether as receiver or as transmitter.

In my first experiments a battery of four Bunsen cells was included in a telephone circuit of small resistance. At the sending station, which we shall call A, an arrangement was made whereby different lengths of various kinds and thicknesses of wire could be inserted in the circuit. At first these wires were inserted by being soldered to the copper terminals, in order to keep clear of loose contacts; but it was afterwards found that all error arising from this source could be avoided by simply clamping the wires firmly between two binding screws. This method, from its greater convenience, was therefore afterwards adopted. To the middle of the inserted wire, and at right angles to it, was attached a fine iron wire about 15 inches long, the other end of which was connected to the centre of the parchment disc of a mechanical telephone. When this wire was stretched moderately tight the transmitting arrangement was complete. At the receiving station, which we shall call B, an ordinary double-ear Bell telephone of small resistance was employed.

When a fine iron wire about 9 inches long was inserted in the circuit at A, any musical sound uttered into the mechanical telephone was most distinctly reproduced at B. Speech could also be

so transmitted, and in one case I managed to do this so successfully that a listener was able to write down an unknown sentence spoken into the mechanical telephone.

I found also that certain lengths of the fine iron wire suited certain voices better than others.

Still using the same fine wire at A, I removed one by one the cells from the battery till only one remained, and found that this did not produce any marked effect on the intensity of the sound as heard at B. The last cell was then removed, and the circuit joined up, and even then a loud sound uttered at A could be faintly heard at B. The only, at least obvious, reason for this effect appears to be, that the vibrations of the iron wire across the lines of force due to the earth's magnetism produces a current sufficiently strong and variable to work a telephone.

The battery being again included in the circuit, a horse-shoe magnet with the line of its poles at right angles to the wire was suspended over and quite close to the wire. This caused the sounds uttered at A to be reproduced at B with distinctly greater intensity than when no magnet was present. To make sure of this a continuous note was sounded at A, and the magnet alternately removed and brought up to the wire. When this was done a marked rise and fall in the intensity of the sound heard in the receiving telephone was observed.

It is to be noticed that, in the case of the fine iron wire, the sounds appeared to be transmitted equally well whether the wire attached to the mechanical telephone was joined to the middle of, and at right angles to, the inserted wire, or to its end, and in the same direction as itself. This observation is of importance, as it does away, in great part at least, with the idea that the effect is produced by variation in the resistance of the fine wire caused merely by the bending to and fro at its points of attachment to the circuit wires.

When the fine iron wire was replaced, all other things remaining the same, by a thick iron wire (No. 12), which had been previously rubbed with a magnet, the sound heard at B was very faint, although audible. It came out, however, very distinctly where the iron wire was heated by a flame to a dull red heat.

With a thickish platinum wire inserted at A the sound produced at B was very faint; but on putting in about twelve inches of fine

platinum wire, the result was almost as good as that obtained from the fine iron wire itself. This is rather a puzzling result to explain, as it cannot arise from magnetisation of the metal, as is probably the case, in part a least, with the iron wire.

Fine wires of copper and aluminum transmitted no sound whatever, although they were treated precisely in the same way as the iron and plantinum wires.

A short strip of cobalt inserted in place of the fine wire at A gave a very distinct result, although the sound was not so loud as in the case of the iron wire. This, however, may be due to smallness of the vibrations arising from the stiffness of the strip.

From all these experiments it is obvious that, some how or other, rapid variations of the current strength are produced in the circuit, and the problem is to explain how these variations are produced. Now the current strength can only be varied in two ways—either by varying the electro-motive force or by altering the resistance of the circuit; and no doubt, in this case, both ways come into play to a certain extent. The former comes into play, inasmuch as the electromotive force will be varied by the motion of the fine wire in the magnetic field caused by itself and by the earth's magnetism. The latter will also come into play, inasmuch as change of resistance will arise from at least three distinct sources. These are (1) varying magnetisation in the wire, especially the iron wire, produced by strain; (2) variations in the temperature of the wire caused by the cooling effect of the air as the wire vibrates; (3) alterations of the resistance caused by the varying strain to which a vibrating wire is exposed. It is possible to arrive at something like a numerical estimate of this last cause of alteration for any particular note transmitted; for, knowing the mass of the vibrating wire, its initial tension, its number of vibrations per second, we can find the variations in the strain to which it is subject; and if we can know how varying strain is connected with change of resistance, as may be got from Sir William Thomson's recent paper on that subject, we have all the elements necessary for making the calculation.

It is right that I should mention that, in making the experiments, I had the valuable aid of Professor Chrystal in the twofold way of helping me to make exact observations, and of suggesting various changes of experiment to bring out or eliminate particular effects.

12. Further Note on Graphitoid Boron and the Production of Nitride of Boron. By R. Sydney Marsden, D.Sc. F.R.S.E., &c.

This note is a continuation of the paper on the "Preparation and Properties of Pure Graphitoid and Adamantine Boron," by Dr R. M. Morrison and myself, published in the Transactions of this Society, vol. xxviii. p. 689, and its object is to correct a mistake which we have made in giving the properties of this substance. We say—

1st, It is not oxidised by air at a white heat, even superficially.

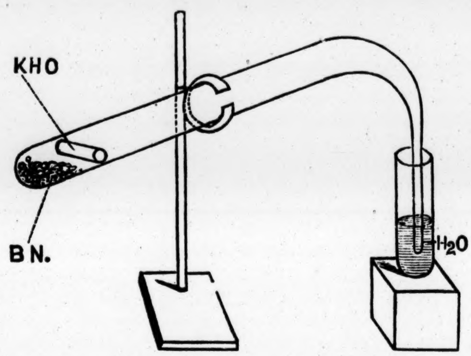
2d, It does not alloy with platinum at a white heat.

I have again prepared a quantity of this substance and examined its properties under Professor Wöhler, and I find that the above statements are wrong. A very fine film of oxide does form on the surface of this substance when heated on platinum foil over the blowpipe, and this film, although very thin and difficult to observe, is sufficient to prevent all further action of the air, and also to prevent its combining with the platinum. This misled us when we previously examined it. If, however, a quantity of it be placed on a piece of platinum foil, and the foil folded over it and pressed down so as to exclude all the air, then on heating it intensely before the blow-pipe the boron at once combines with the platinum and perforates it.

In that paper also we mention a slatish-grey powder which we found surrounding the metal on opening the porcelain crucible. I have made an examination of this powder, and find it consists of a mixture of nitride of boron and amorphous boron, chiefly, however, of nitride of boron, which is formed by the superfluous boron uniting with the nitrogen of the air. If the experiment is prolonged over many hours the whole of the amorphous boron is converted into this nitride, and the powder is then white. The way in which I tested for the nitride was as follows:—A portion of the powder was heated in a hard glass-tube, with solid caustic potash to convert the nitrogen into ammonia, when the following reaction takes place—

$$2BN + 6KHO = 2NH_3 + B_2O_3 + 3K_2O.$$

The ammonia is led into a small tube containing water, and tested for in the usual way. In a number of other similar experiments in



which boron was heated with the different metals, this nitride of boron was always found in the crucible surrounding the metal at the end of the experiment.

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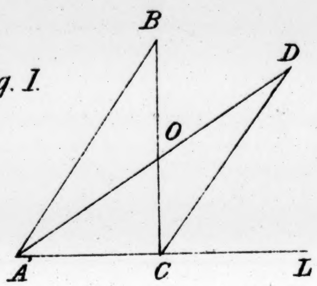

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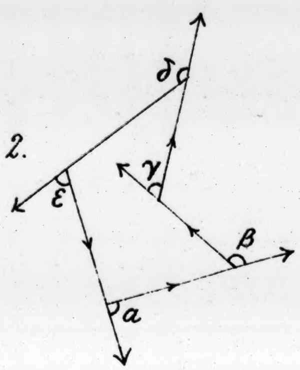

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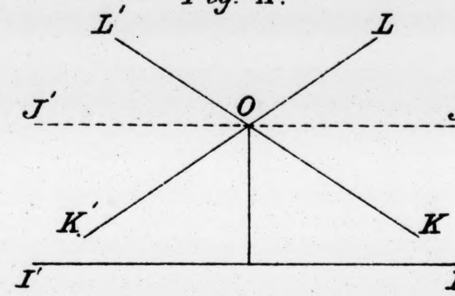

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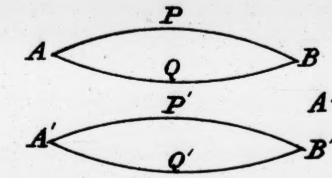

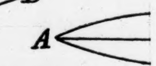

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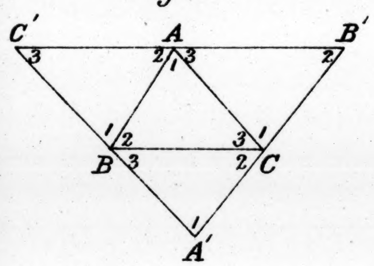

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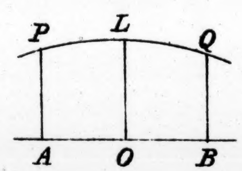

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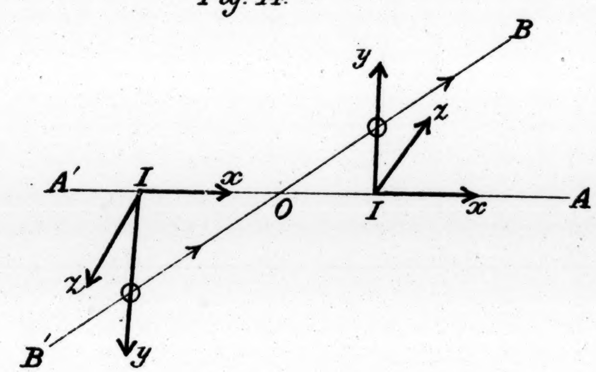

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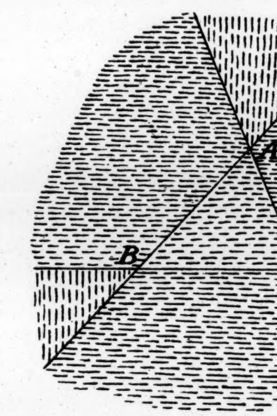

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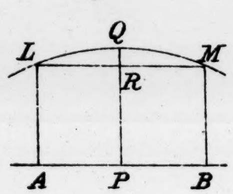

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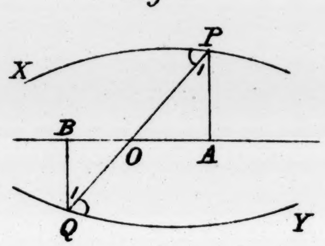

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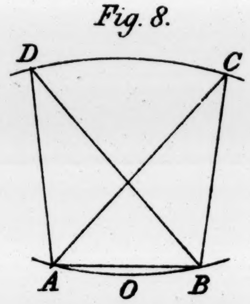

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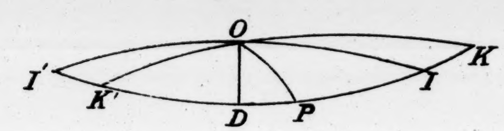

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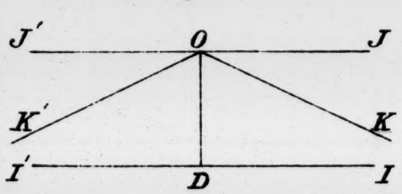

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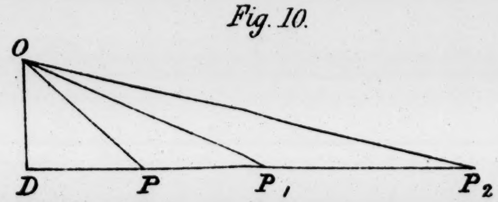

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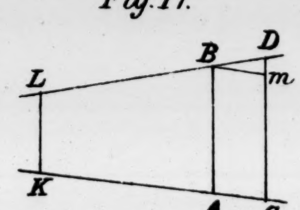

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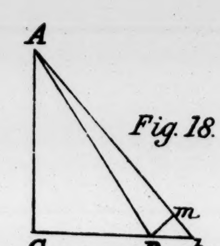

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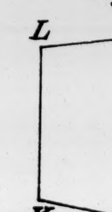

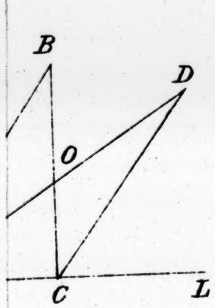

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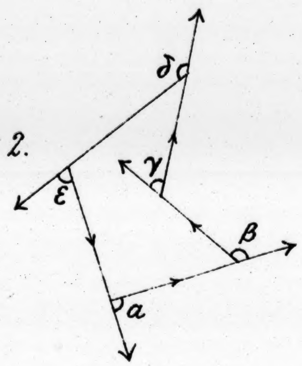

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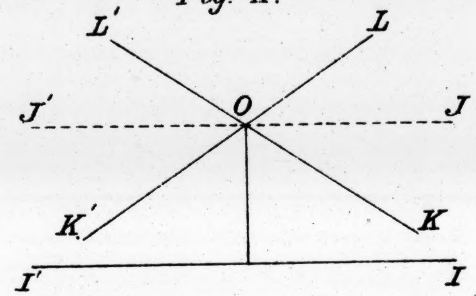

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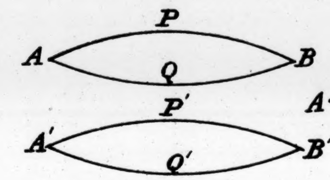

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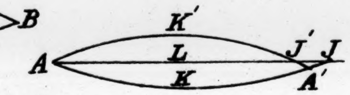

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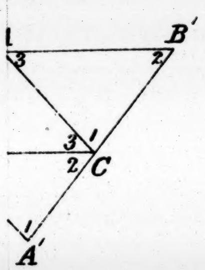

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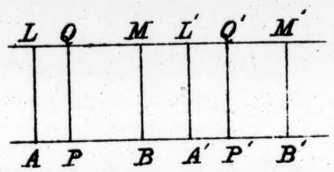

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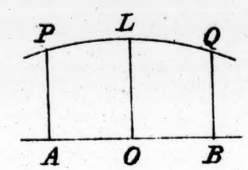

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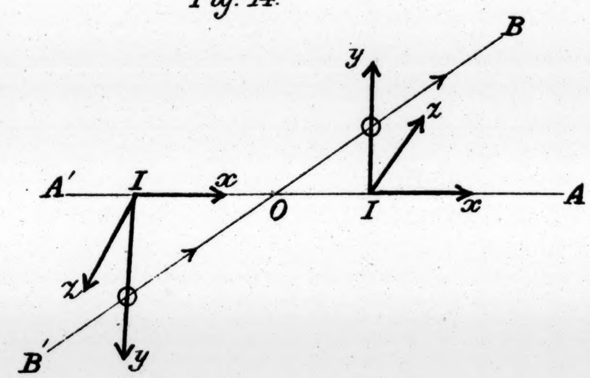

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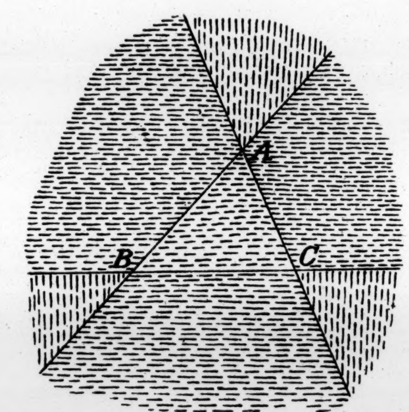

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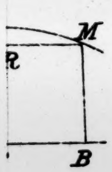

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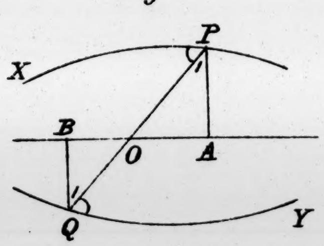

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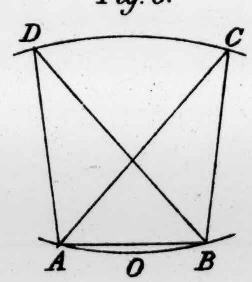

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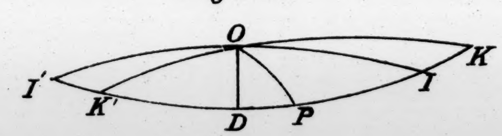

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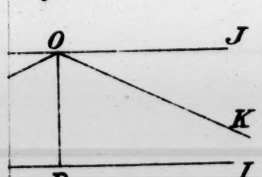

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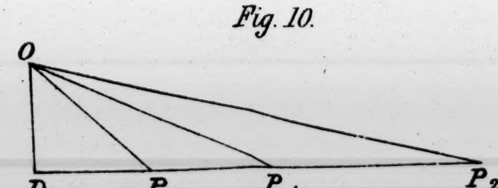

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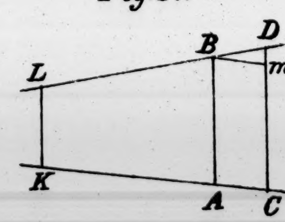

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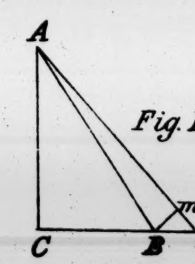

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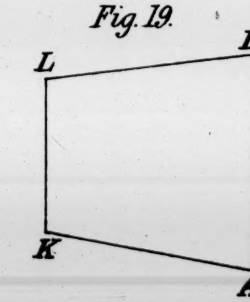

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